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In This Issue

In a stimulating address on history as myth, WILLIAM MCNEILL challenges us to overcome parochialism and to strive for world understanding by taking a broad, ecumenical view of history. As if in response to this appeal, the three articles that complete the issue all treat the impact of ideas and programs generated by Western countries on societies at the periphery of Western civilization.

Russia lies closest to the West, and historians of that country have long used a European model of social estates as a category of analysis. They have assumed that an estate order existed in Russia since the Middle Ages and that the order decayed in the nineteenth century. GREGORY FREEZE disputes these notions. In the first thorough analysis of the estate concept in Russian history, Freeze argues that social estates, in the sense the term has commonly been used by Russian historians, emerged as recently as the late eighteenth century and were gaining strength and significance at the time that most scholars have said they were in decline.

ROBERT FRYKENBERG looks at the reception of West European education in South India and challenges the notion that it was an alien imposition upon a hapless people by arrogant and imperious rulers. He finds that the demand for modern education came from within Indian society, advanced by a native elite that had previously acquired mastery of Persian in order to serve the Mughals and that would not be denied access to English language and modern learning.

DAVID WEBER looks to another point of contact between cultures: the American West. He asks why the frontier thesis of Frederick Jackson Turner exerted little influence on historians of the Borderlands school, in spite of the urgings of the leader of that school, Herbert Eugene Bolton, that his students apply Turner's ideas in their work. Weber notes that, although neglected by the Boltonians, Turner's interpretations received attention from two other groups of historians who worked on the western borderlands: (1) historians of Latin America and the U.S. who have done broad comparative frontier studies and (2) practitioners of the new social history who have examined in detail the society of the Hispanic frontier. They have seldom mentioned Turner explicitly, however, because the heart of his thesis—that the frontier altered national character and institutions and made them more democratic—has not seemed to apply to Mexico.

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Mythistory, or Truth, Myth, History, and Historians

WILLIAM H. McNEILL

Myth and history are close kin inasmuch as both explain how things got to be the way they are by telling some sort of story. But our common parlance reckons myth to be false while history is, or aspires to be, true. Accordingly, a historian who rejects someone else's conclusions calls them mythical, while claiming that his own views are true. But what seems true to one historian will seem false to another, so one historian's truth becomes another's myth, even at the moment of utterance.

A century and more ago, when history was first established as an academic discipline, our predecessors recognized this dilemma and believed they had a remedy. Scientific source criticism would get the facts straight, whereupon a conscientious and careful historian needed only to arrange the facts into a readable narrative to produce genuinely scientific history. And science, of course, like the stars above, was true and eternal, as Newton and Laplace had demonstrated to the satisfaction of all reasonable persons everywhere.

Yet, in practice, revisionism continued to prevail within the newly constituted historical profession, as it had since the time of Herodotus. For a generation or two, this continued volatility could be attributed to scholarly success in discovering new facts by diligent work in the archives; but early in this century thoughtful historians began to realize that the arrangement of facts to make a history involved subjective judgments and intellectual choices that had little or nothing to do with source criticism, scientific or otherwise.

In reacting against an almost mechanical vision of scientific method, it is easy to underestimate actual achievements. For the ideal of scientific history did allow our predecessors to put some forms of bias behind them. In particular, academic historians of the nineteenth century came close to transcending older religious controversies. Protestant and Catholic histories of post-Reformation Europe ceased to be separate and distinct traditions of learning—a transformation nicely illustrated in the Anglo-American world by the career of Lord Acton, a Roman Catholic who became Regius Professor of History at Cambridge and editor of the first *Cambridge Modern History*. This was a great accomplishment. So was the accumulation of an enormous fund of exact and reliable data through painstaking

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source criticism that allowed the writing of history in the western world to assume a new depth, scope, range, and precision as compared to anything possible in earlier times. No heir of that scholarly tradition should scoff at the faith of our predecessors, which inspired so much toiling in archives.

Yet the limits of scientific history were far more constricting than its devotees believed. Facts that could be established beyond all reasonable doubt remained trivial in the sense that they did not, in and of themselves, give meaning or intelligibility to the record of the past. A catalogue of undoubted and indubitable information, even if arranged chronologically, remains a catalogue. To become a history, facts have to be put together into a pattern that is understandable and credible; and when that has been achieved, the resulting portrait of the past may become useful as well—a font of practical wisdom upon which people may draw when making decisions and taking action.

Pattern recognition of the sort historians engage in is the chef d'oeuvre of human intelligence. It is achieved by paying selective attention to the total input of stimuli that perpetually swarm in upon our consciousness. Only by leaving things out, that is, relegating them to the status of background noise deserving only to be disregarded, can what matters most in a given situation become recognizable. Suitable action follows. Here is the great secret of human power over nature and over ourselves as well. Pattern recognition is what natural scientists are up to; it is what historians have always done, whether they knew it or not.

Only some facts matter for any given pattern. Otherwise, useless clutter will obscure what we are after: perceptible relationships among important facts. That and that alone constitutes an intelligible pattern, giving meaning to the world, whether it be the world of physics and chemistry or the world of interacting human groups through time, which historians take as their special domain. Natural scientists are ruthless in selecting aspects of available sensory inputs to pay attention to, disregarding all else. They call their patterns theories and inherit most of them from predecessors. But, as we now know, even Newton's truths needed adjustment. Natural science is neither eternal nor universal; it is instead historical and evolutionary, because scientists accept a new theory only when the new embraces a wider range of phenomena or achieves a more elegant explanation of (selectively observed) facts than its predecessor was able to do.

No comparably firm consensus prevails among historians. Yet we need not despair. The great and obvious difference between natural scientists and historians is the greater complexity of the behavior historians seek to understand. The principal source of historical complexity lies in the fact that human beings react both to the natural world and to one another chiefly through the mediation of symbols. This means, among other things, that any theory about human life, if widely believed, will alter actual behavior, usually by inducing people to act as if the theory were true. Ideas and ideals thus become self-validating within remarkably elastic limits. An extraordinary behavioral motility results. Resort to symbols, in effect, loosened up the connection between external reality and human responses, freeing us from instinct by setting us adrift on a sea of uncertainty. Human beings thereby acquired a new capacity to err, but also to change, adapt,

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and learn new ways of doing things. Innumerable errors, corrected by experience, eventually made us lords of creation as no other species on earth has ever been before.

The price of this achievement is the elastic, inexact character of truth, and especially of truths about human conduct. What a particular group of persons understands, believes, and acts upon, even if quite absurd to outsiders, may nonetheless cement social relations and allow the members of the group to act together and accomplish feats otherwise impossible. Moreover, membership in such a group and participation in its sufferings and triumphs give meaning and value to individual human lives. Any other sort of life is not worth living, for we are social creatures. As such we need to share truths with one another, and not just truths about atoms, stars, and molecules but about human relations and the people around us.

Shared truths that provide a sanction for common effort have obvious survival value. Without such social cement no group can long preserve itself. Yet to outsiders, truths of this kind are likely to seem myths, save in those (relatively rare) cases when the outsider is susceptible to conversion and finds a welcome within the particular group in question.

The historic record available to us consists of an unending appearance and dissolution of human groups, each united by its own beliefs, ideals, and traditions. Sects, religions, tribes, and states, from ancient Sumer and Pharaonic Egypt to modern times, have based their cohesion upon shared truths—truths that differed from time to time and place to place with a rich and reckless variety. Today the human community remains divided among an enormous number of different groups, each espousing its own version of truth about itself and about those excluded from its fellowship. Everything suggests that this sort of social and ideological fragmentation will continue indefinitely.

Where, in such a maelstrom of conflicting opinions, can we hope to locate historical truth? Where indeed?

Before modern communications thrust familiarity with the variety of human idea-systems upon our consciousness, this question was not particularly acute. Individuals nearly always grew up in relatively isolated communities to a more or less homogeneous world view. Important questions had been settled long ago by prophets and sages, so there was little reason to challenge or modify traditional wisdom. Indeed there were strong positive restraints upon any would-be innovator who threatened to upset the inherited consensus.

To be sure, climates of opinion fluctuated, but changes came surreptitiously, usually disguised as commentary upon old texts and purporting merely to explicate the original meanings. Flexibility was considerable, as the modern practice of the U.S. Supreme Court should convince us; but in this traditional ordering of intellect, all the same, outsiders who did not share the prevailing orthodoxy were shunned and disregarded when they could not be converted. Our predecessors' faith in a scientific method that would make written history absolutely and universally true was no more than a recent example of such a belief system. Those who embraced it felt no need to pay attention to ignoramuses who

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had not accepted the truths of "modern science." Like other true believers, they were therefore spared the task of taking others' viewpoints seriously or wondering about the limits of their own vision of historical truth.

But we are denied the luxury of such parochialism. We must reckon with multiplex, competing faiths—secular as well as transcendental, revolutionary as well as traditional—that resound amongst us. In addition, partially autonomous professional idea-systems have proliferated in the past century or so. Those most important to historians are the so-called social sciences—anthropology, sociology, political science, psychology, and economics—together with the newer disciplines of ecology and semeiology. But law, theology, and philosophy also pervade the field of knowledge with which historians may be expected to deal. On top of all this, innumerable individual authors, each with his own assortment of ideas and assumptions, compete for attention. Choice is everywhere: dissent turns into cacaphonous confusion; my truth dissolves into your myth even before I can put words on paper.

The liberal faith, of course, holds that in a free marketplace of ideas, Truth will eventually prevail. I am not ready to abandon that faith, however dismaying our present confusion may be. The liberal experiment, after all, is only about two hundred and fifty years old, and on the appropriate world-historical time scale that is too soon to be sure. Still, confusion is undoubted. Whether the resulting uncertainty will be bearable for large numbers of people in difficult times ahead is a question worth asking. Iranian Muslims, Russian communists, and American sectarians (religious and otherwise) all exhibit symptoms of acute distress in face of moral uncertainties, generated by exposure to competing truths. Clearly, the will to believe is as strong today as at any time in the past; and true believers nearly always wish to create a community of the faithful, so as to be able to live more comfortably, insulated from troublesome dissent.

The prevailing response to an increasingly cosmopolitan confusion has been intensified personal attachment, first to national and then to subnational groups, each with its own distinct ideals and practices. As one would expect, the historical profession faithfully reflected and helped to forward these shifts of sentiment. Thus, the founding fathers of the American Historical Association and their immediate successors were intent on facilitating the consolidation of a new American nation by writing national history in a WASPish mold, while also claiming affiliation with a tradition of Western civilization that ran back through modern and medieval Europe to the ancient Greeks and Hebrews. This version of our past was very widely repudiated in the 1960s, but iconoclastic revisionists felt no need to replace what they attacked with any architectonic vision of their own. Instead, scholarly energy concentrated on discovering the history of various segments of the population that had been left out or ill-treated by older historians: most notably women, blacks, and other ethnic minorities within the United States and the ex-colonial peoples of the world beyond the national borders.

Such activity conformed to our traditional professional role of helping to define collective identities in ambiguous situations. Consciousness of a common past, after all, is a powerful supplement to other ways of defining who "we" are. An oral

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tradition, sometimes almost undifferentiated from the practical wisdom embodied in language itself, is all people need in a stable social universe where in-group boundaries are self-evident. But with civilization, ambiguities multipled, and formal written history became useful in defining "us" versus "them." At first, the central ambiguity ran between rulers and ruled. Alien conquerors who lived on taxes collected from their subjects were at best a necessary evil when looked at from the bottom of civilized society. Yet in some situations, especially when confronting natural disaster or external attack, a case could be made for commonality, even between taxpayers and tax consumers. At any rate, histories began as king lists, royal genealogies, and boasts of divine favor—obvious ways of consolidating rulers' morale and asserting their legitimacy vis-à-vis their subjects.

Jewish history emphasized God's power over human affairs, narrowing the gap between rulers and ruled by subjecting everybody to divine Providence. The Greeks declared all free men equal, subject to no one, but bound by a common obedience to law. The survival value of both these visions of the human condition is fairly obvious. A people united by their fear and love of God have an ever-present help in time of trouble, as Jewish history surely proves. Morale can survive disaster, time and again; internal disputes and differences diminish beneath the weight of a shared subjection to God. The Greek ideal of freedom under law is no less practical in the sense that willing cooperation is likely to elicit maximal collective effort, whether in war or peace.

Interplay between these two ideals runs throughout the history of Western civilization, but this is not the place to enter into a detailed historiographical analysis. Let me merely remark that our professional heritage from the liberal and nationalist historiography of the nineteenth century drew mainly on the Greek, Herodotean model, emphasizing the supreme value of political freedom within a territorially defined state.

World War I constituted a catastrophe for that liberal and nationalist vision of human affairs, since freedom that permitted such costly and lethal combat no longer seemed a plausible culmination of all historic experience. Boom, bust, and World War II did nothing to clarify the issue, and the multiplication of subnational historiographies since the 1950s merely increased our professional confusion.

What about truth amidst all this weakening of old certainties, florescence of new themes, and widening of sensibilites? What really and truly matters? What should we pay attention to? What must we neglect?

All human groups like to be flattered. Historians are therefore under perpetual temptation to conform to expectation by portraying the people they write about as they wish to be. A mingling of truth and falsehood, blending history with ideology, results. Historians are likely to select facts to show that we—whoever "we" may be—conform to our cherished principles: that we are free with Herodotus, or saved with Augustine, or oppressed with Marx, as the case may be. Grubby details indicating that the group fell short of its ideals can be skated over or omitted entirely. The result is mythical: the past as we want it to be, safely simplified into a contest between good guys and bad guys, "us" and "them." Most national history and most group history is of this kind, though the intensity of chiaroscuro varies

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greatly, and sometimes an historian turns traitor to the group he studies by setting out to unmask its pretensions. Groups struggling toward self-consciousness and groups whose accustomed status seems threatened are likely to demand (and get) vivid, simplified portraits of their admirable virtues and undeserved sufferings. Groups accustomed to power and surer of their internal cohesion can afford to accept more subtly modulated portraits of their successes and failures in bringing practice into conformity with principles.

Historians respond to this sort of market by expressing varying degrees of commitment to, and detachment from, the causes they chronicle and by infusing varying degrees of emotional intensity into their pages through particular choices of words. Truth, persuasiveness, intelligibility rest far more on this level of the historians' art than on source criticism. But, as I said at the beginning, one person's truth is another's myth, and the fact that a group of people accepts a given version of the past does not make that version any truer for outsiders.

Yet we cannot afford to reject collective self-flattery as silly, contemptible error. Myths are, after all, often self-validating. A nation or any other human group that knows how to behave in crisis situations because it has inherited a heroic historiographical tradition that tells how ancestors resisted their enemies successfully is more likely to act together effectively than a group lacking such a tradition. Great Britain's conduct in 1940 shows how world politics can be redirected by such a heritage. Flattering historiography does more than assist a given group to survive by affecting the balance of power among warring peoples, for an appropriately idealized version of the past may also allow a group of human beings to come closer to living up to its noblest ideals. What is can move toward what ought to be, given collective commitment to a flattering self-image. The American civil rights movement of the fifties and sixties illustrates this phenomenon amongst us.

These collective manifestations are of very great importance. Belief in the virtue and righteousness of one's cause is a necessary sort of self-delusion for human beings, singly and collectively. A corrosive version of history that emphasizes all the recurrent discrepancies between ideal and reality in a given group's behavior makes it harder for members of the group in question to act cohesively and in good conscience. That sort of history is very costly indeed. No group can afford it for long.

On the other hand, myths may mislead disastrously. A portrait of the past that denigrates others and praises the ideals and practice of a given group naively and without restraint can distort a people's image of outsiders so that foreign relations begin to consist of nothing but nasty surprises. Confidence in one's own high principles and good intentions may simply provoke others to resist duly accredited missionaries of the true faith, whatever that faith may be. Both the United States and the Soviet Union have encountered their share of this sort of surprise and disappointment ever since 1917, when Wilson and Lenin proclaimed their respective recipes for curing the world's ills. In more extreme cases, mythical, self-flattering versions of the past may push a people toward suicidal behavior, as Hitler's last days may remind us.

More generally, it is obvious that mythical, self-flattering versions of rival groups'

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pasts simply serve to intensify their capacity for conflict. With the recent quantum jump in the destructive power of weaponry, hardening of group cohesion at the sovereign state level clearly threatens the survival of humanity; while, within national borders, the civic order experiences new strains when subnational groups acquire a historiography replete with oppressors living next door and, perchance, still enjoying the fruits of past injustices.

The great historians have always responded to these difficulties by expanding their sympathies beyond narrow in-group boundaries. Herodotus set out to award a due meed of glory both to Hellenes and to the barbarians; Ranke inquired into what really happened to Protestant and Catholic, Latin and German nations alike. And other pioneers of our profession have likewise expanded the range of their sympathies and sensibilities beyond previously recognized limits without ever entirely escaping, or even wishing to escape, from the sort of partisanship involved in accepting the general assumptions and beliefs of a particular time and place.

Where to fix one's loyalties is the supreme question of human life and is especially acute in a cosmopolitan age like ours when choices abound. Belonging to a tightly knit group makes life worth living by giving individuals something beyond the self to serve and to rely on for personal guidance, companionship, and aid. But the stronger such bonds, the sharper the break with the rest of humanity. Group solidarity is always maintained, at least partly, by exporting psychic frictions across the frontiers, projecting animosities onto an outside foe in order to enhance collective cohesion within the group itself. Indeed, something to fear, hate, and attack is probably necessary for the full expression of human emotions; and ever since animal predators ceased to threaten, human beings have feared, hated, and fought one another.

Historians, by helping to define "us" and "them," play a considerable part in focusing love and hate, the two principal cements of collective behavior known to humanity. But myth making for rival groups has become a dangerous game in the atomic age, and we may well ask whether there is any alternative open to us.

In principle the answer is obvious. Humanity entire possesses a commonality which historians may hope to understand just as firmly as they can comprehend what unites any lesser group. Instead of enhancing conflicts, as parochial historiography inevitably does, an intelligible world history might be expected to diminish the lethality of group encounters by cultivating a sense of individual identification with the triumphs and tribulations of humanity as a whole. This, indeed, strikes me as the moral duty of the historical profession in our time. We need to develop an ecumenical history, with plenty of room for human diversity in all its complexity.

Yet a wise historian will not denigrate intense attachment to small groups. That is essential to personal happiness. In all civilized societies, a tangle of overlapping social groupings lays claim to human loyalties. Any one person may therefore be expected to have multiple commitments and plural public identities, up to and including membership in the human race and the wider DNA community of life on planet Earth. What we need to do as historians and as human beings is to recognize this complexity and balance our loyalties so that no one group will be

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able to command total commitment. Only so can we hope to make the world safer for all the different human groups that now exist and may come into existence.

The historical profession has, however, shied away from an ecumenical view of the human adventure. Professional career patterns reward specialization; and in all the well-trodden fields, where pervasive consensus on important matters has already been achieved, research and innovation necessarily concentrate upon minutiae. Residual faith that truth somehow resides in original documents confirms this direction of our energies. An easy and commonly unexamined corollary is the assumption that world history is too vague and too general to be true, that is, accurate to the sources. Truth, according to this view, is only attainable on a tiny scale when the diligent historian succeeds in exhausting the relevant documents before they exhaust the historian. But as my previous remarks have made clear, this does not strike me as a valid view of historical method. On the contrary, I call it naive and erroneous.

All truths are general. All truths abstract from the available assortment of data simply by using words, which in their very nature generalize so as to bring order to the incessantly fluctuating flow of messages in and messages out that constitutes human consciousness. Total reproduction of experience is impossible and undesirable. It would merely perpetuate the confusion we seek to escape. Historiography that aspires to get closer and closer to the documents—all the documents and nothing but the documents—is merely moving closer and closer to incoherence, chaos, and meaninglessness. That is a dead end for sure. No society will long support a profession that produces arcane trivia and calls it truth.

Fortunately for the profession, historians' practice has been better than their epistemology. Instead of replicating confusion by paraphrasing the totality of relevant and available documents, we have used our sources to discern, support, and reinforce group identities at national, transnational, and subnational levels and, once in a while, to attack or pick apart a group identity to which a school of revisionists has taken a scunner.

If we can now realize that our practice already shows how truths may be discerned at different levels of generality with equal precision simply because different patterns emerge on different time-space scales, then, perhaps, repugnance for world history might diminish and a juster proportion between parochial and ecumenical historiography might begin to emerge. It is our professional duty to move toward ecumenicity, however real the risks may seem to timid and unenterprising minds.

With a more rigorous and reflective epistemology, we might also attain a better historiographical balance between Truth, truths, and myth. Eternal and universal Truth about human behavior is an unattainable goal, however delectable as an ideal. Truths are what historians achieve when they bend their minds as critically and carefully as they can to the task of making their account of public affairs credible as well as intelligible to an audience that shares enough of their particular outlook and assumptions to accept what they say. The result might best be called mythistory perhaps (though I do not expect the term to catch on in professional circles), for the same words that constitute truth for some are, and always will be,

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myth for others, who inherit or embrace different assumptions and organizing concepts about the world.

This does not mean that there is no difference between one mythistory and another. Some clearly are more adequate to the facts than others. Some embrace more time and space and make sense of a wider variety of human behavior than others. And some, undoubtedly, offer a less treacherous basis for collective action than others. I actually believe that historians' truths, like those of scientists, evolve across the generations, so that versions of the past acceptable today are superior in scope, range, and accuracy to versions available in earlier times. But such evolution is slow, and observable only on an extended time scale, owing to the self-validating character of myth. Effective common action can rest on quite fantastic beliefs. *Credo quia absurdum* may even become a criterion for group membership, requiring initiates to surrender their critical faculties as a sign of full commitment to the common cause. Many sects have prospered on this principle and have served their members well for many generations while doing so.

But faiths, absurd or not, also face a long-run test of survival in a world where not everyone accepts any one set of beliefs and where human beings must interact with external objects and nonhuman forms of life, as well as with one another. Such "foreign relations" impose limits on what any group of people can safely believe and act on, since actions that fail to secure expected and desired results are always costly and often disastrous. Beliefs that mislead action are likely to be amended; too stubborn an adherence to a faith that encourages or demands hurtful behavior is likely to lead to the disintegration and disappearance of any group that refuses to learn from experience.

Thus one may, as an act of faith, believe that our historiographical myth making and myth breaking is bound to cumulate across time, propagating mythistories that fit experience better and allow human survival more often, sustaining in-groups in ways that are less destructive to themselves and to their neighbors than was once the case or is the case today. If so, ever-evolving mythistories will indeed become truer and more adequate to public life, emphasizing the really important aspects of human encounters and omitting irrelevant background noise more efficiently so that men and women will know how to act more wisely than is possible for us today.

This is not a groundless hope. Future historians are unlikely to leave out blacks and women from any future mythistory of the United States, and we are unlikely to exclude Asians, Africans, and Amerindians from any future mythistory of the world. One hundred years ago this was not so. The scope and range of historiography has widened, and that change looks as irreversible to me as the widening of physics that occurred when Einstein's equations proved capable of explaining phenomena that Newton's could not.

It is far less clear whether in widening the range of our sensibilities and taking a broader range of phenomena into account we also see deeper into the reality we seek to understand. But we may. Anyone who reads historians of the sixteenth and seventeenth centuries and those of our own time will notice a new awareness of social process that we have attained. As one who shares that awareness, I find it

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impossible not to believe that it represents an advance on older notions that focused attention exclusively, or almost exclusively, on human intentions and individual actions, subject only to God or to a no less inscrutable Fortune, while leaving out the social and material context within which individual actions took place simply because that context was assumed to be uniform and unchanging.

Still, what seems wise and true to me seems irrelevant obfuscation to others. Only time can settle the issue, presumably by outmoding my ideas and my critics' as well. Unalterable and eternal Truth remains like the Kingdom of Heaven, an eschatological hope. Mythistory is what we actually have—a useful instrument for piloting human groups in their encounters with one another and with the natural environment.

To be a truth-seeking mythographer is therefore a high and serious calling, for what a group of people knows and believes about the past channels expectations and affects the decisions on which their lives, their fortunes, and their sacred honor all depend. Formal written histories are not the only shapers of a people's notions about the past; but they are sporadically powerful, since even the most abstract and academic historiographical ideas do trickle down to the level of the commonplace, if they fit both what a people want to hear and what a people need to know well enough to be useful.

As members of society and sharers in the historical process, historians can only expect to be heard if they say what the people around them want to hear—in some degree. They can only be useful if they also tell the people some things they are reluctant to hear—in some degree. Piloting between this Scylla and Charybdis is the art of the serious historian, helping the group he or she addresses and celebrates to survive and prosper in a treacherous and changing world by knowing more about itself and others.

Academic historians have pursued that art with extraordinary energy and considerable success during the past century. May our heirs and successors persevere and do even better!

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The *Soslovie* (Estate) Paradigm and Russian Social History

GREGORY L. FREEZE

IN A SERIES OF LECTURES delivered nearly a century ago, Vasilii O. Kliuchevskii, the dean of prerevolutionary Russian historians, complained that scholars had studied particular groups in Russia but not the larger social structure.[1] Since then historians have written many more studies of specific groups but have yet to reconsider traditional assumptions and ideas about prerevolutionary Russian society that still pervade the historiography. The persistence of antiquated sociological views, for the most part formulated in the second half of the nineteenth century, stands in striking contrast to other fields of European history, where stratification, terminology, and patterns of social change have been the focus of a continuing and much-needed debate. But in Russian historiography the fundamental conceptions of prerevolutionary historians have been uncritically preserved, even in the most sophisticated studies of individual groups, and historians have casually used such basic terms as "class" and "estate" interchangeably.[2]

At the core of prerevolutionary historiography is a cluster of ideas about Russia's peculiar system of "estates" (*sosloviia*; in the singular, *soslovie*) and their development in the eighteenth and nineteenth centuries. The traditional conception of the social structure posited the existence of four main estates (nobility, clergy, townspeople, and peasantry)—a model not unlike the formal structure of medieval Europe. Such a simple system, which seemed logical enough in preindustrial Russia, enjoyed widespread acceptance in both scholarly and popular writings. To quote one historian-journalist, writing in 1859, "Every estate has its own role in the state: the clergy pray, the nobles serve in war and peace, the peasants plough and feed the people, and the merchants are the means that provide each with what it needs."[3] The idea of four estates was, moreover, explicitly recognized in Russian

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[1] Kliuchevskii, *Istoriia soslovii v Rossii*, reprinted in his *Sochineniia*, 6 vols. (Moscow, 1956–59), 6: 303. *Istoriia soslovii v Rossii* was first presented as a series of lectures in 1886 and published in 1887.

[2] For the case of Soviet historiography, see the example cited in Gary M. Hamburg, *Politics of the Russian Nobility, 1881–1905* (New Brunswick, N.J., 1984), 5. But Western studies are no different. See, for instance, Terence Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge, 1968), 3–4, 339–41, 359.

[3] M. P. Pogodin, as quoted in N. P. Barsukov, *Zhizn' i trudy M. P. Pogodina*, 16 (St. Petersburg, 1902): 591.

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law—above all, in the *Digest of Laws* published in 1832, the first new law code in Russia in nearly two centuries.[4] But broader popularization of the concept of four estates did not come until somewhat later, chiefly through the "state school" of Russian historiography, which emerged in the mid-nineteenth century and formulated many of the basic conceptions that have dominated the field ever since.[5] Most striking here is the notion that Russian estates developed very differently from their nominal peers in the West: they appeared much later (in the eighteenth century—just as estates were dissolving in Western Europe) and represented not a medieval legacy but a social order consciously established by the all-powerful state. Subsequently, "liberal" historians writing in the late nineteenth century, who were politically hostile to autocracy, perpetuated the earlier view that the state played a dominant role in the formation of the estates, but they inverted the assessment of the state's positive influence to argue that it was oppressive and responsible for the "unnatural" and retarded development of Russian society.[6]

These notions still inform contemporary Western scholarship on Russia's prerevolutionary social history. Thus, it is commonly assumed that the Russian estates, in contrast to those in Western Europe, did not even appear until the eighteenth century, that they represented the creations of the absolutist state (rather than the legacy of a feudal past), and that they lacked the corporate vitality characteristic of European estates.[7] The *sosloviia* were abortive as "genuine" estates capable of restraining autocracy, yet, according to the traditional Western view, they gravely retarded social and economic development, because they arrested the

[4] The first edition of the *Digest of Laws* divided the population, on the basis of "differences in rights," into four groups: nobility, clergy, urban residents (*gorodskie obyvateli*), and rural residents (*sel'skie obyvateli*); *Svod zakonov rossiiskoi imperii*, 9 (St. Petersburg, 1832), bk. 1, art. 2.

[5] For the emergence of the four-estate interpretation among leading figures of the state school (above all, the Hegelians K. D. Kavelin, B. N. Chicherin, and S. M. Solov'ev), see Kliuchevskii, *Sochineniia*, 6: 301. For an early statement, see Kavelin's master's dissertation of the mid-1840s; "Osnovnye nachala sudoustroistva i grazhdanskogo sudoproizvodstva ot Ulozheniia do Uchrezhdeniia o guberniiakh," in Kavelin, *Sochineniia*, pt. 1 (Moscow, 1859): 39–304.

[6] For classics of prerevolutionary liberal scholarship, see A. A. Kizevetter, *Posadskaia obshchina v Rossii XVIII v.* (St. Petersburg, 1903); and P. N. Miliukov, *Ocherki po istorii russkoi kul'tury*, 1 (6th edn., Moscow, 1918): 219, 248–49.

[7] For typical statements on the appearance of the estates, see Jerome Blum, "Russia," in *European Landed Elites in the Nineteenth Century* (Baltimore, Md., 1977), 71; Marc Raeff, *The Well-Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600–1800* (New Haven, Conn., 1983), 238–39; and Dietrich Geyer, "'Gesellschaft' als staatliche Veranstaltung," *Jahrbücher für Geschichte Osteuropas*, new ser., 14 (1966): 22. For examples from prerevolutionary Russian historiography, see M. F. Vladimirskii-Budanov, *Obzor istorii russkogo prava* (3d edn., St. Petersburg, 1900), 250; and A. D. Gradovskii, *Nachala russkogo gosudarstvennogo prava* (Moscow, 1875), reprinted in his *Sobranie sochinenii*, 7 (St. Petersburg, 1901): 200–54. Also see specialized monographs on individual *sosloviia*. The most striking exception is Kliuchevskii, who used *soslovie* in the elastic sense of "social category" and applied it to all periods of Russian history, from Kievan Rus' to the nineteenth century. See his *Sochineniia*, 6: 276–466. On the estates as the creation of an absolutist state, see Raeff, *Well-Ordered Police State*, 238–39; Blum, "Russia," 72; Neil Weissman, *Reform in Tsarist Russia: The State Bureaucracy and Local Government, 1900–1914* (New Brunswick, N.J., 1981), 7; Miliukov, *Ocherki*, 219; N. M. Korkunov, *Russkoe gosudarstvennoe pravo*, 1 (7th edn., St. Petersburg, 1909): 274–75; and Gradovskii, *Sobranie sochinenii*, 187. On the deficient vitality of Russian estates, see Vladimirskii-Budanov, *Obzor*, 250; Miliukov, *Ocherki*, 248–49; Korkunov, *Russkoe gosudarstvennoe pravo*, 275; Raeff, *Well-Ordered Police State*, 238–39; Manfred Hildermeier, "Bürgertum und Stadt in Russland, 1760–1860: Rechtliche Lage und soziale Struktur" (Habilitationsschrift, Berliner Freie-Universität, 1983), esp. 660–79; and D. Geyer, "Staatsausbau und Sozialverfassung: Probleme des russischen Absolutismus," *Cahiers du monde russe et soviétique*, 7 (1966): 366–77.

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natural processes of change and growth. It was only the gradual "emancipation" of society—from bureaucratic controls, from moribund estates—that permitted more dynamic growth in the last half of the nineteenth century. That process accelerated steadily from the time of the Great Reforms in the 1860s, which began to dismantle the estate structure and unleashed a transformation of Russian estates into classes—in a paradigm familiar to European social historians.[8]

These interpretations also appear in Soviet historiography, although there are, to be sure, some differences between Soviet and Western scholarship. In particular, Soviet historians consider the estates an intrinsic part of the "feudal society" of pre-Petrine Russia and mark their existence from the ninth to the nineteenth centuries. In that scheme, however, the eighteenth century does not denote merely another epoch but the "culmination" of the estate system in Russia.[9] Moreover, given the assumptions of Marxist historiography, Soviet scholars naturally feel discomfited by use of a juridical term to describe social and economic groups and have tended to employ a hybrid term, *klass-soslovie*, in an effort to subsume the "economic substructure" within the traditional estate model.[10] Still, Soviet historians in essence accept the premises of prerevolutionary and Western scholarship: they assign the state a major, if not decisive, role in shaping social development; they perceive a basic aggregation and unification of medieval categories into social estates in the eighteenth century; and they treat the period 1861–1917 as an ineluctable evolution from *sosloviia* to classes.[11]

This essay seeks to reexamine the *soslovie* paradigm—first, by investigating the conception and development of the term *soslovie* and, second, by studying its

[8] For typical statements of this conception, see Jerome Blum, *The End of the Old Order in Europe* (Princeton, N.J., 1978), 440–41, "Russia," 79–80, 92; Leopold Haimson, "Conclusion: Observations on the Politics of the Russian Countryside (1905–14)," in Haimson, ed., *The Politics of Rural Russia, 1905–14* (Bloomington, Ind., 1979), 276; Weissman, *Reform in Tsarist Russia*, 7–8; and Thomas C. Owens, *Capitalism and Politics in Russia* (Cambridge, 1981), 174. The Western model for such a conceptualization goes back to Marx, Weber, and Sombart and is still the main construct in European social history. See, for example, Roland Mousnier, *Social Hierarchies* (New York, 1973); Jürgen Kocka, "Stand-Klasse-Organisation: Strukturen sozialer Ungleichheit in Deutschland vom späten 18. bis zum frühen 20. Jahrhundert im Aufriss," in Hans-Ulrich Wehler, ed., *Klassen in der europäischen Sozialgeschichte* (Göttingen, 1979), 140; and Werner Conze, "Das Spannungsfeld von Staat und Gesellschaft im Vormärz," in Conze, ed., *Staat und Gesellschaft im deutschen Vormärz* (Stuttgart, 1962), 207–69.

[9] For a cursory discussion, see, in particular, "Soslovie," *Sovetskaia istoricheskaia entsiklopediia*, 13 (Moscow, 1971): 347–51.

[10] For instances of such usage, see N. P. Eroshkin, *Krepostnoe samoderzhavie i ego politicheskie instituty* (Moscow, 1981), 25–26; N. I. Pavlenko, "K voprosu ob evoliutsii dvorianstva v XVII–XVIII vv.," in *Voprosy genezisa kapitalizma v Rossii: Sbornik statei* (Leningrad, 1960), 54; and A. P. Korelin, *Dvorianstvo v poreformennoi Rossii, 1861–1904 gg.* (Moscow, 1979), 54. For the "Leninist" origins of the term, see M. T. Beliavskii, "Klassy i sosloviia feodal'nogo obshchestva v Rossii v svete Leninskogo naslediia," *Vestnik Moskovskogo gosudarstvennogo universiteta*, seriia istoriia, 1970, no. 2: 68.

[11] For example, see N. D. Zol'nikova, *Soslovnye problemy vo vzaimootnosheniiakh tserkvi i gosudarstva v Sibiri (XVIII v.)* (Novosibirsk, 1981), 5 n.; E. V. Anisimov, *Podatnaia reforma Petra I* (Leningrad, 1982), 135; Z. Ia. Boiarshinova, "O formirovanii sosloviia gosudarstvennykh krest'ian v Sibiri (XVIII–pervaia polovina XIX vv.)," *Trudy Tomskogo gosudarstvennogo universiteta*, 1964, no. 177: 44–55; P. G. Ryndziunskii, "Soslovno-podatnaia reforma 1775 g. i gorodskoe naselenie," in *Obshchestvo i gosudarstvo feodal'noi Rossii* (Moscow, 1975), 86; S. I. Smetanin, "Razlozhenie soslovii i formirovanie klassovoi struktury gorodskogo naseleniia Rossii v 1800–1861 gg.," *Istoricheskie zapiski*, 102 (1978): 153–82; A. S. Nifontov, "Formirovanie klassov burzhuaznogo obshchestva v russkom gorode vtoroi poloviny XIX v.," *Istoricheskie zapiski*, 54 (1955): 241; and Pavlenko, "K voprosu ob evoliutsii dvorianstva," 54. For a more cautious statement of the "aggregation" thesis, see A. A. Preobrazhenskii, "Ob evoliutsii klassovo-soslovnogo stroia v Rossii," in *Obshchestvo i gosudarstvo feodal'noi Rossii*, 69.

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application to Russian social history. It will be argued here that the modern notion of *soslovie* arose only in the early nineteenth century, that the estate system was dynamic and still actively developing (not disintegrating) in the nineteenth century, and that the resilience of this social order contravenes the customary assumption of an inexorable transition from estate to class in postreform and revolutionary Russia.

THE ABSENCE OF A STUDY of the development of the term *soslovie* constitutes an important lacuna in the existing historiography.[12] Pre-Petrine Russia knew neither *soslovie* (in the sense of estate) nor its equivalent, for the social structure consisted of numerous, small groups and lacked collective terms for legal aggregation. The complicated social terminology of Muscovite Russia is highly suggestive of its social complexity. One lexicon for the period records nearly five hundred separate social categories to denote different ranks and statuses.[13] Although in rare instances the sources hint of larger aggregates roughly akin to "estates," such terms were so uncommon that they only emphasize the peculiar, fragmented structure of medieval Russian society. The most prominent and distinct social category was the *chin* (rank), but it applied only to the privileged service classes, not the rest of society, and, significantly enough, even the privileged classes lacked a collective name.[14] Splintered into a plethora of categories, hierarchically ranked by a system

[12] The deficiency is aptly noted by Dietrich Geyer. See his "'Gesellschaft' als staatliche Veranstaltung," 35 n. The examination of terminology is important, as sophisticated analyses have repeatedly demonstrated. For a good discussion of the theoretical aspects, see Reinhart Koselleck, ed., *Historische Semantik und Begriffsgeschichte* (Stuttgart, 1979). For rich examples of such work, see the multivolume *Geschichtliche Grundbegriffe* (five volumes to date).

[13] G. E. Kochin, *Materialy dlia terminologicheskogo slovaria drevnei Rossii* (Moscow, 1937), 436–39.

[14] For example, a collective petition of the nobility, dated 1660, speaks of "four ranks: ecclesiastical, service, commercial, and agricultural"; A. A. Novosel'skii, "Pobegi krest'ian i kholopov i ikh sysk v Moskovskom gosudarstve vtoroi poloviny XVII veka," in *Trudy Instituta istorii RANION*, 1 (Moscow, 1926): 352. An identical formulation appeared in another collective supplication of 1657; V. N. Storozhev, "Dva chelobit'ia (k bibliografii materialov dlia istorii russkogo dvorianstva)," *Bibliograficheskie zapiski*, 1892, no. 1: 14. Gunther Stökl has argued that, at least in the early seventeenth century, *chin* roughly approximated the meaning of "estate," that some writers rendered *chin* with the Polish *stan* (meaning "estate"), and that the subsequent absence of connotations of "estate" need not reflect usage in earlier periods; Stökl, "Gab es im Moskauer Staat Stände?" *Jahrbücher für Geschichte Osteuropas*, new ser., 11 (1963): 321–42. H.-J. Torke has rightly noted, however, that the chance renderings of foreigners cannot carry much weight (and Stökl relied heavily on such evidence), particularly since the *chin* category was so unlike the European "estate." See Torke, *Die staatsbedingte Gesellschaft im Moskauer Reich: Zar und Zemlja in der altrussischen Herrschaftsverfassung, 1613–1689* (Leiden, 1974), 271–83. Moreover, in marked contrast to the traditional paradigm of estates in European history, the basic structure of stratification in Russia followed a very different principle—not privilege or residence but the form of state obligation, which determined the basic division of society into *sluzhilye* (service), *osviashchennye* (clerical), *tiaglye* (tax-bearing), and *netiaglye* (tax-exempt) categories. Seventeenth-century documents ordinarily restricted the application of *chin* to the privileged elite, a complex of twenty-three ecclesiastical and civil ranks. For the rest of society, references were either missing altogether or reduced to vague, ambiguous formulae like "the Russian land." See Torke, *Staatsbedingte Gesellschaft*, 273; and Werner Philipp, "Zur Frage nach der Existenz altrussischer Stände," *Forschungen zur osteuropäischen Geschichte*, 27 (1980): 64–76. According to Kliuchevskii, *sluzhilye liudi* ("servicemen"), the term that recurs in the historiography, was unknown in Muscovite sources; "Terminologiia russkoi istorii," in *Sochineniia*, 6: 157. Still more anachronistic for the pre-Petrine social order is the term *sluzhiloe soslovie*. See, for example, N. P. Pavlov-Sil'vanskii, *Gosudarevye sluzhilye liudi* (2d edn., St. Petersburg, 1909); and Richard Pipes, *Russia under the Old Regime* (London, 1974), 86.

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of precedence (*mestnichestvo*), the military and service classes hardly represented a cohesive, unified social stratum. On the contrary, they included not only the boyar magnates and titled aristocrats, Moscow and provincial (*gorodovye*) noblemen, but also a substantial number of peripheral groups, such as the *odnodvortsy* (single homesteaders) and the *strel'tsy* (urban musketeers).

This "orderless order" changed substantially between the late seventeenth and early nineteenth centuries, generating significant alterations in terminology and conceptualization. The traditional *chin*, to be sure, did not suddenly disappear. Rather, it initially acquired broader meaning (to encompass larger social categories) and remained the term most often used until the 1760s, even recurring sporadically thereafter. But *chin* plainly did not suffice to express the notion of larger estates, partly because of its traditional restriction to ecclesiastical and secular service classes, partly because of its newer usage in the Petrine Table of Ranks, and partly because it denoted specifically the male servitor but not his dependents.[15] The need for a new social terminology became particularly apparent in the 1760s, when state officials gave consideration to the task of developing a *tiers état*. Although one writer, Denis I. Fonvizin, tried to render *tiers état* by a modification of the traditional *chin* (as *tretii chin*), most advisers felt the need for a newer, more expressive phrase. Thus, one Catherinean servitor, Grigorii N. Teplov, variously rendered *tiers état* with a barbarism from the German, *srednii shtat liudei* ("middle 'state' of people"), or a phrase borrowed from the Polish, *tretii stan liudei* ("third 'estate' of people"). The term that actually prevailed, *srednii rod liudei* ("middle sort of people"), was so devoid of social specificity that it could hardly suggest "estate," yet it became the term used by the Legislative Commission of 1767–68. In the next few decades a range of terms appeared, all reflecting the need for a more expressive social lexicon.[16]

[15] See H. A. Bennett, "Evolution of the Meanings of *Chin*: An Introduction to the Russian Institution of Rank Ordering and Niche Assignment from the Time of Peter the Great's Table of Ranks to the Boshevik Revolution," *California Slavic Studies*, 10 (1977): 1–43. For examples of usage in the 1760s, see N. I. Novikov, *Truten'* (1769), reprinted in P. N. Berkov, ed., *Satiricheskie zhurnaly N. I. Novikova* (Moscow, 1951), 164; S. E. Desnitskii, "Predstavlenie o uchrezhdenii zakonodatel'noi suditel'noi i nakazatel'noi vlasti v rossiiskoi imperii," reprinted in I. Ia. Shchipanov, ed., *Izbrannye proizvedeniia russkikh myslitelei vtoroi poloviny XVIII veka*, 2 vols. (Moscow, 1952), 1: 299. Recognition of the broader meaning of *chin* was still registered in a mid-nineteenth-century dictionary; *Slovar' tserkovno-slavianskogo i russkogo iazyka*, 4 vols. (St. Petersburg, 1847), 4: 440. For a typical example of the restricted meaning of *chin*—for the non–poll tax groups—where the responsible domain (*vedomstvo*) as much as the group itself is implied, see the reference to *chin* in the 1767 *nakaz* from the Senate to the Legislative Commission; *Sbornik Imperatorskogo russkogo istoricheskogo obshchestva* (hereafter, *SIRIO*), 148 vols. (St. Petersburg, 1867–1916), 43: 16. In brief, Peter's Table of Ranks precipitated a transformation of the meaning of *chin* from office to personal title. For example, D. I. Fonvizin distinguished between *zvanie* (as *dolzhnost'*) and *chin* (as *stepen' chesti, na kotorye gosudar' dostoinykh liudei vozvodit*). See his "Opyt rossiiskogo soslovnika" (1783), reprinted in G. P. Makogonenko, ed., *Russkaia literatura XVIII v.* (Leningrad, 1970), 352. For a good discussion of this change, see H.-J. Torke, "Das russische Beamtentum in der ersten Hälfte des 19. Jahrhunderts," *Forschungen zur osteuropäischen Geschichte*, 13 (1967): 51–53. Desnitskii used *zvanie* to refer to nobles, merchants, and the like, but as individuals, not as members of social collectivities; "Predstavlenie o uchrezhdenii," in Shchipanov, *Izbrannye proizvedeniia*, 1: 297.

[16] For discussions of the general interest in fashioning a third estate in Russia, see Hildermeier, "Bürgertum und Stadt in Russland," pt. 1; David M. Griffiths, "Eighteenth-Century Perception of Backwardness: Projects for the Creation of a Third Estate in Catherinean Russia," *Canadian-American Slavic Studies*, 13 (1979): 452–72; Robert E. Jones, "Jakob Sievers, Enlightened Reform, and the

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By the last quarter of the eighteenth century, however, the traditional meaning of *chin*, or of newer concoctions, was superseded by two terms that were used in formal law for the duration of the imperial period: *sostoianie* and *zvanie*.[17] Although both these words remained current and could even be employed interchangeably, *sostoianie* became the predominant term. At first it tended to connote little more than "condition" or "status," with no specific attribution to a particular group, but eventually it came to denote "legal status group" and to refer to any social category, privileged or otherwise. The word's predominance persisted, especially in formal governmental usage, where it remained the basic legal term until the end of the *ancien régime*.[18]

Development of a 'Third Estate' in Russia," *Russian Review*, 36 (1977): 424–37; and S. M. Troitskii, "Dvorianskie proekty sozdaniia 'tret'ego china,'" in *Obshchestvo i gosudarstvo feodal'noi Rossii*, 226–36. Fonvizin's term occurred in his translation of M. de Boulard's "Précis de la liberté de la noblesse française et sur l'utiliteé d'un tiers état." See "Kratkoe izlozhenie o vol'nosti frantsuzskogo dvorianstva i o pol'ze tret'ego china," in *Arkhiv kn. Vorontsova*, 26 (Moscow, 1882): 315–24. For Teplov's essay, wrongly attributed to M. V. Lomonosov, see "Neizvestnoe sochinenie Lomonosova 'O kommertsii,'" *Beseda*, 1872, no. 1: 167. Teplov used the Polish *stan* in his phrase "*tretii stan liudei, t.e., meshchanskii*" ("the third estate of people, that is, lower townsmen"). The term *stan* appeared sporadically in the eighteenth century. See, for example, V. N. Tatishchev's statement that the nobility represents "glavnyi i chestneishii stan gosudarstva"; Tatishchev, *Leksikon*, pt. 2 (St. Petersburg, 1793): 120, in P. K. Alefirenko, "Sotsial'no-politicheskie vozzreniia V. N. Tatishcheva," *Voprosy istorii*, 1951, no. 10: 106. The formal proposal to create a new urban society bore the title "Proekt zakonov o pravakh srednego roda zhitelei"; *SIRIO*, 36: 179–232. By extension, some contemporaries used the phrase "*nizhnii rod*" to denote the lower social strata; Desnitskii, "Predstavlenie o uchrezhdenii," in Shchipanov, *Izbrannye proizvedeniia*, 1: 318. For the usage by G. Korob'in at the Legislative Commission in 1767, see Shchipanov, *Izbrannye proizvedeniia*, 2: 63. One interesting transitional product was the hybrid term *chinosostoianie*, which sought to express the idea of both official rank and social status. For examples of such usage in 1801, see the reform proposal "Proekt dlia razdeleniia ulozheniia rossiiskogo," in A. N. Radishchev, *Polnoe sobranie sochinenii*, 3 vols. (Moscow, 1938–52), 3: 167. The word "class" (*klass*) also made its appearance by this time, drawn from the neutral sense of category found in classical literature. See, for example, the address of a deputy at the Legislative Commission in 1767; *SIRIO*, 4: 180. For Mikhail M. Speranskii's discussion of *klass liudei rabochikh*, see his "Vvedenie k ulozheniiu gosudarstvennykh zakonov" (1809), in Speranskii, *Plan gosudarstvennogo preobrazovaniia* (Moscow, 1905), 65. For the Western development of this term, which appeared first in the generalized sense of "category" in the seventeenth century, see Peter Calvert, *The Concept of Class: An Historical Introduction* (London, 1982). For specific cases, see R. Mousnier, "D'Aguessau et le tournant des Ordres aux classes sociales," *Revue d'histoire économique et sociale*, 49 (1971): 449–64; and R. J. Morris, *Class and Class Consciousness in the Industrial Revolution, 1780–1850* (London, 1979). It is a matter of considerable significance that, at the very time that Western European society was shifting from the language of "estate" to that of "class," Russia was still groping for an indigenous equivalent of *état* and *Stand*.

[17] The term *zvanie* enjoyed fairly widespread use but tended to be closely associated with specific occupational groups; it was a more elastic version of *chin* and could be applied to the unprivileged as well as elites, to a broad status group like the clergy (*dukhovnoe zvanie*) but also to particular occupations within a larger group. For instance, a 1760 law referred to various *zvaniia* of serfs; *Polnoe sobranie zakonov rossiiskoi imperii*, 1st ser. (hereafter, *PSZ* 1), 45 vols. (St. Petersburg, 1830), 15: 11166. For additional evidence and use of the term, see *Rukopisnyi leksikon pervoi poloviny XVIII v.* (Leningrad, 1964), 118; I. Piskarev, *Drevnie gramoty i akty Riazanskogo kraia* (St. Petersburg, 1854), 170; "Rech' Gosudariny Imp. Ekateriny II," *Chteniia v obshchestve istorii i drevnostei rossiiskikh*, 1862, bk. 2, sect. 5, pp. 187–88; *SIRIO*, 36: 179; Novikov, *Truten'* (1769), reprinted in Berkov, *Satiricheskie zhurnaly*, 164.

[18] Note, for example, P. A. Tret'iakov's reference to "*liudi vysshego i nizshego sostoianiia*" in his "Slovo o proisshestvii i uchrezhdenii universitetov v Evrope na gosudarstvennykh izhdiveniiakh" (1763); Shchipanov, *Izbrannye proizvedeniia*, 1: 338 n. In the 1760s, A. I. Polenov wrote that Russia had no "srednee sostoianie" (middle estate); Polenov, "O krepostnom sostoianii krest'ian v Rossii," *Russkii arkhiv*, 1865, no. 3: 297–316. On *sostoianie*, see, for example, Novikov, *Truten'* (1770), in Berkov, *Satiricheskie zhurnaly*, 212. For use of the term in a state decree of February 20, 1803, see *PSZ* 1, 27: 20620. For an 1818 proposal of political reform, see N. K. Shil'der, *Imperator Aleksandr I*, 4 (2d edn., St. Petersburg, 1905): 522. The Committee of the Sixth of December 1826, which prepared a draft manifesto on the various social categories and their needs, used the word *sostoianie* to label the four

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But, even as *sostoianie* acquired predominance in formal law, it had to compete with *soslovie*, a term far more pregnant in semantic potential and significance. The etymology and lexical history of this word provide significant information on the history of social structure and thought in prerevolutionary Russia. Although the meaning of its root has been the subject of some disagreement, the word initially connoted nothing more than "gathering" or "assembly," a social collectivity with no hint of formal organization. It was in this vague sense of assembly that the term appeared until the last third of the eighteenth century. Toward the end of that century, however, it developed a more abstract connotation, referring not merely to "assembly" in the narrower sense but also to "organized society" or "community." Over the next several decades the connotation of "community" broadened, so that *soslovie* acquired the supplementary meaning of "society" in the sense of a formal social organization with a distinct body of members.[19]

That additional meaning led to a semantic breakthrough in the first decade of the nineteenth century, when the term also came to denote "constituted body" and "legal estate." *Soslovie*, to mean a "constituted body," was applied literally to formal governmental institutions—for example, to the Senate and even to the quasi-parliamentary institutions proposed by Mikhail M. Speranskii, who combined the notions of "assembly" and "constituted body" in an effort to describe what he wanted to create.[20] Concomitantly, *soslovie* also appeared, in unofficial usage, in the

main estates of the realm. To quote the committee's proposal ("Proekt dopolnitel'nogo zakona o sostoianiiakh s prinadlezhashchim k nemu prilozheniiam"): "By the fundamental laws of our state, four main *sostoianiia* have been erected in it: the nobility, the clergy, the [urban] citizenry [*grazhdanstvo*], and the peasantry"; *SIRIO*, 90: 363. This amorphous, but desirably elastic, term also appeared in the *Digest of Laws*; *Svod zakonov*, vol. 9, bk. 1, art. 2.

[19] On earlier etymology, see I. I. Sreznevskii, *Materialy dlia slovaria drevnerusskogo iazyka*, 3 (Moscow, 1956), 822; M. Vasmer, *Etimologicheskii slovar' russkogo iazyka*, 3 (Moscow, 1973): 726; A. G. Preobrazhenskii, *Etymological Dictionary of the Russian Language*, 3 (New York, 1951), 360. The definition of "assembly" (*sobranie*) as a social collectivity appears in the dictionary of P. A. Alekseev, first published in the late eighteenth century and reprinted often thereafter; Alekseev, *Tserkovnyi slovar'*, 5 pts. (4th edn., St. Petersburg, 1817–19). *Soslovie* in this sense appears in the works of Feofan (Prokopovich), an early eighteenth-century clerical writer; Makogonenko, *Russkaia literatura*, 49. M. M. Shcherbatov used *soslovie* in the sense of "society" in 1767 during a speech at the Legislative Commission in which he argued that the lower townsmen (*meshchane*) did not form a true *soslovie* or community, even though many townsmen called themselves *meshchane*. He proposed that measures be taken to create such a *soslovie* at the urban level—meaning, evidently, an organized community in the city (not a hereditary legal estate in the later sense). See *SIRIO*, 8: 66–67. Similarly, in a volume published in 1763, the bishop of Riazan', Palladii (Raev), expressed the hope that his collection of sermons would be of benefit to the entire "Christian community" (*soslovie khristianskoe*). See Palladii (Raev), *Sobranie raznykh pouchitel'nykh slov, skazyvaemykh v vysokotorzhestvennye dni, v nekotorye gospodskie i bogorodichnye prazdniki*, 1 (St. Petersburg, 1763): *list* 2v. In both cases usage is transitional, the connotation somewhat broader and more generalized than the traditional "assembly" (*sobranie*). Much broader still was the later usage recorded in the second edition of the Academy dictionary, which defined *soslovie* not only as "assembly" (*sobranie*) but also as "a community composed of a definite number of members" (*obshchestvo, sostoiashchee iz izvestnogo chisla chlenov*). See *Slovar' Akademii rossiiskoi*, 6 vols. (Moscow, 1806–22), 6: 395. Significantly, the first edition of this dictionary contained only the traditional meaning of "assembly" and omitted the newer, more formal definition. See *Slovar' Akademii rossiiskoi*, 6 vols. (1st edn., St. Petersburg, 1789–94), 5: 544. Illustrative of the newer usage was an essay in 1802 by V. V. Popugaev, who used *soslovie* as a synomym for *obshchestvo* in describing the literary society to which he belonged. See his "Rech' na den' chrezvychainogo sobraniia iiunia 15–go dnia 1802 goda," reprinted in I. Ia. Shchipanov, ed., *Russkie prosvetiteli (ot Radishcheva do dekabristov)*, 2 vols. (Moscow, 1966), 1: 389.

[20] For the Senate example, see *PSZ* 1, 27: 20405. Speranskii's 1809 reform proposal used *soslovie* to describe "legislative body" (*zakonodatel'noe soslovie*) and "judicial body" (*sudebnoe soslovie*); here *soslovie* is roughly equivalent to "institution." Speranskii, *Plan gosudarstvennogo preobrazovaniia* (Moscow, 1905),

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sense of a legal, formally constituted estate. Catherine II had used "society" (*obshchestvo*) to denote the corporate bodies she established in 1785 for nobles and townspeople, and the additional meaning of *soslovie* as "society" made it suitable for describing these new entities, particularly since it included notions of "constituted body" and "institution."[21] That lexical development offers an instructive comparison with European models: like *état* and *Stand*, the Russian *soslovie* combined the etymological ideas of "state institution" and "social group"—with the significant difference that this process transpired centuries later and took on a far more complex and dynamic form.[22]

This usage of *soslovie* applied most accurately to townspeople and the nobility, who in fact possessed such status and organization after the 1785 Catherinean reforms.[23] But usage steadily broadened, and by the 1820s *soslovie* could be applied to all social groups, not only the four main estates but also other distinctive social categories.[24] By 1847 *soslovie* as a term for a legal estate had appeared in an Academy of Sciences dictionary, which defined the word as "a category of people with a specific occupation, distinguished from others by their special rights and

32–33, and *Proekty i zapiski*, ed. S. N. Valk (Moscow, 1961), 178–89. A similar usage appeared in A. P. Kunitsyn's *Pravo estestvennoe*, written in 1818–20, in which *soslovie* denoted formal institution; Shchipanov, *Russkie prosvetiteli*, 2: 330. The same usage, significantly, recurs in the 1832 *Digest of Laws* in the section on the clergy, in which *soslovie* is used to denote not the clerical estate but ecclesiastical institutions—monasteries, churches, and episcopal residences—as juridical persons; *Svod zakonov*, vol. 9, bk. 1, art. 2, section 4.

[21] *PSZ* 1, 22: 16187 (April 21, 1785). The term *obshchestvo* was also used collectively for the merchants (*kupechestvo*) in the first edition of the Academy dictionary. See *Slovar' Akademii rossiiskoi* (1st edn.), 3: 1072. An instructive transitional usage appears in the works of I. P. Pnin, who used *soslovie* to refer to governmental institutions and, in his pedagogical guide on education, to denote "the main *gosudarstvennye* [state] *sosloviia* in Russia, that is, the agricultural (*zemledel'cheskoe*), townsman, noble, and clerical" estates. See Shchipanov, *Russkie prosvetiteli*, 1: 197, 205, 213.

[22] Although the meaning of *état* is self-evident, it should be noted that the German *Stand* arose in the fifteenth century and derived from the word for "state." See H. L. Mikoletzky, *Grundbegriffe der Geschichte* (Gütersloh, 1964), 371–77.

[23] Although there were exceptions (for example, Pnin's use of *soslovie*), usage in 1800–10 tended to be restricted to the townspeople and, especially, the nobility. An 1807 decree refers to the nobility and townspeople as "two *sosloviia*," and in the second edition of the Academy dictionary *soslovie* appears only in the definitions of the nobility and townspeople, not the peasantry and clergy (for whom the word *sostoianie* was used). See *PSZ* 1, 19: 22148; and *Slovar' Akademii rossiiskoi*, 2: 39, 284; 3: 405, 951. The use of *soslovie* to describe the nobility was especially common. See, for example, the various circulars of the Ministry of Education; S. V. Rozhdestvenskii, "Soslovnyi vopros v russkikh universitetakh v pervoi polovine XIX v.," *Zhurnal Ministerstva narodnogo prosveshcheniia*, 1907, no. 9: 90–91. For later usage in A. S. Pushkin's works, see *Slovar' iazyka Pushkina*, 4 (Moscow, 1961): 244.

[24] An anonymous petitioner of 1825 and the head of the Third Section in 1827 both used *soslovie* in reference to the peasantry. For the text of the former, see P. G. Ryndziunskii, *Gorodskoe grazhdanstvo doreformennoi Rossii* (Moscow, 1958), 130. For the text of the latter, see "Graf A. Kh. Benkendorf o Rossii v 1827–1830 gg.," *Krasnyi arkhiv*, 1929, no. 37: 152. More flexible was the usage in 1810 by N. S. Mordvinov, who inflated the number of estates from four to five: "The best division of the people is by *sosloviia*—nobility, clergy, merchants, townspeople, and peasant"; *Arkhiv grafov Mordvinovykh*, 9 (St. Petersburg, 1903), 54. Still more striking was the application of *soslovie* to such groups as doctors and bureaucrats. The government itself used *soslovie* to describe doctors, for the legal medical oath referred to them as a *soslovie*—in the sense of corporate group; Nancy M. Frieden, *Russian Physicians in an Era of Reform and Revolution, 1856–1905* (Princeton, N.J., 1981), 327. For characterization of the *chinovniki* (state officials) as a *soslovie*, see the Third Section report of 1827 and later writings by such members of the intelligentsia as V. G. Belinskii, A. I. Herzen, and N. G. Chernyshevskii; "Graf A. Kh. Benkendorf," 150; V. G. Belinskii, *Sobranie sochinenii*, 2 (Moscow, 1948): 834–35; A. I. Gertsen, *Byloe i Dumy*, in *Sochineniia*, 4 (Moscow, 1956): 252; and N. G. Chernyshevskii, *Polnoe sobranie sochinenii*, 10 (Moscow, 1951): 101.

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obligations."[25] Although rarely used in formal law, *soslovie* had nonetheless entered the vocabulary of educated people by the 1840s and 1850s, chiefly through the influence of the state school, which superimposed on the conception of a legal estate its own special views on state and social development.[26]

But the term continued to acquire new connotations—in particular, an overtone of "caste," which emphasized the endogamous and cultural separateness that significantly deepened differences in legal status.[27] Indeed, by the 1860s, when Vladimir I. Dal' composed his classic dictionary of the Russian language, he felt obliged to include "caste" as one of the defining words in his entry on *soslovie*.[28] That additional connotation, distinctly pejorative, emerged in the very midst of the Great Reforms, when state and society sought to regenerate Russia and to eliminate all that impeded its growth and development. As the reformers made their comparisons between Russia and Western Europe, it is hardly surprising that they cast a critical eye on hereditary *sosloviia*. As they sought the gradual dismantlement of this social order, *soslovie* reached the final stage in its semantic development, becoming the primary descriptive term for the social system in late imperial Russia.

This excursus into *Begriffsgeschichte* affords several important observations. First, it is anachronistic to use the term *soslovie*, at least without qualification, before the early nineteenth century. To be sure, its belated appearance does not necessarily preclude its application to the preceding period. A social phenomenon can precede the appearance of a term to describe it, as happened, for example, with "intelligentsia."[29] But the appearance of the term *soslovie* did add a decisive element, not only testifying to contemporary perceptions of the new order but also reinforcing the barriers separating one group from the other. The new term thus formed a significant part of social reality: to the categories of kinship, occupation, and legal status, *soslovie* added corporateness and distinctive culture. If applied retroactively to the hereditary social strata of the late eighteenth century, *soslovie* must be used only in that sense and carefully distinguished from the later phenomenon of a "constituted body" with a distinctive culture, status, and social identity.

Second, the lexical history of *soslovie* suggests that some traditional assumptions

[25] *Slovar' tserkovno-slavianskogo i russkogo iazyka*, 4: 190.

[26] *Sostoianie* remained the primary term in the *Digest of Laws*. *Soslovie* appeared infrequently and even then often in the sense of formal institution. For a brief summary of this usage, see "Soslovie," *Entsiklopedicheskii slovar' Brokgauz-Efrona*, 60 (St. Petersburg, 1900): 911. Significantly enough, the four-volume index for the second set of *PSZ*, covering the period 1825–81, does not even include *soslovie*.

[27] On the clergy, which attracted criticism as a caste-like order in the late 1850s, see G. L. Freeze, *The Parish Clergy in Nineteenth-Century Russia: Crisis, Reform, Counter-Reform* (Princeton, N.J., 1981), chap. 5. Also see P. A. Valuev's complaint in 1859 that the Russian nobility should be transformed "from a caste into a state *soslovie* in a rational sense"; Valuev, "Dnevnik," *Russkaia starina*, 1891, vol. 72, no. 10: 146.

[28] V. I. Dal', *Tolkovyi slovar' velikorusskogo iazyka*, 4 (3d edn., St. Petersburg, 1904): 409.

[29] For an exhaustive study of the etymology and concept of "intelligentsia," see Otto W. Müller, *Intelligencija: Untersuchungen zur Geschichte eines politischen Schlagwortes*, Frankfurter Abhandlungen zur Slavistik, no. 17 (Frankfurt, 1971); and Alan Pollard, "The Russian Intelligentsia," *California Slavic Studies*, 3 (1964): 8–19.

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about the state's role need to be reconsidered. As the amorphous and shifting development of legal terminology shows, the state's social policy and thought were essentially unsystemic before the early nineteenth century. The reasons for this are many, from the extra-legal status of serfs to the burden of a traditional idiom, but the main point is that the state did not articulate, or seek to establish, a "four-estate model" in the eighteenth century. The Legislative Commission of 1767–68 omitted entire social categories (not just serfs but also clergy), and Catherine's manifestoes of 1785 applied to only two specific groups, the nobility and townspeople, also treated ambiguously.[30] The ambiguity was not simply a matter of the empress's predilections or whims: key figures, "progressive and reactionary," both in and out of government, tended to think in terms of traditional aggregates and to omit whole groups in their enumeration of categories.[31] Nor was it a matter of mere words, for on concrete issues the state treated each group on an *ad hoc* basis. Hence, it formulated policy, modified legal status, and even applied the term *soslovie* without seeking to conflate these groups into a four-estate system. Although some measures (from the poll tax to the creation of constituted bodies) had the effect of rigidifying social categories, these should not be elevated—as historians have traditionally done—into a conception of a deliberate, systematic policy to create a Western order of estates in Russia.

Third, the changes in terminology suggest a different conceptualization of the development of the social structure of prereform Russia, that is, Russia before the Great Reforms of the 1860s. The *soslovie* system—whether defined as legal, social, or cultural-psychological—was maturing, not dissolving, in the first half of the nineteenth century. As the lexical history indicates, consciousness of social categories changed substantially between 1775 and 1850—from an individual (*chin*) to a group (estate) identity, from strictly legal definitions to cultural and caste dimensions. Whatever the viability of Catherinean corporate bodies, social groups were acquiring sharper legal and cultural definition—that is, the *sosloviia* were becoming more, not less, distinct as the Great Reforms approached.[32] In that

[30] It is true that in 1765 Catherine wrote privately of her intent to establish "touttes [sic] sortes d'état dans mon Empire," but neither her own later writing nor specific legislation indicated any systemic conception of the social order; "Pis'ma Imp. Ekateriny II k g-zhi Zhoffrei," *SIRIO*, 1: 276.

[31] An essay from the late eighteenth century by A. N. Radishchev, "Opyt o zakonodatel'stve," is suggestive. He noted that "in Russia the people are divided into three main *sostoianiia*: the clergy, military [*voinstvo*] and civil society [*grazhdanstvo*]." The first two categories represented transparent versions of the traditional *dukhovnyi chin* (ecclesiastical rank) and the *sluzhilye liudi* (service people), while Radishchev's *grazhdanstvo* denoted not *Bürgertum* but sundry estates—nobility, merchants and townsmen, state and crown peasants, serfs, and free people; Radishchev, *Polnoe sobranie sochinenii*, 3: 9. By 1801–02 Radishchev had improved his model. In "Proekt dlia razdeleniia ulozheniia rossiiskogo," he posited five "*sosloviia ili chinosostoianiia*," a phrase indicating a mental transition from ranks to estates. His five estates included the customary four estates and a catch-all fifth: "Categories [*rody*] of people who do not belong to the first four and who have special rights, either temporarily or permanently." And, in contrast to his earlier proposal, Radishchev now declared that the military "do not comprise a true state *soslovie*"; *ibid.*, 167. Similarly, when listing the various estates, two early nineteenth-century writers—Speranskii and N. M. Karamzin—both omitted the clergy from their lists, not from antipathy but from inexperience in systemic social thought. See Karamzin, *Zapiska o drevnei i novoi Rossii* (St. Petersburg, 1914), 127; and Speranskii, *Plan*, 63.

[32] Following Baron S. A. Korf, most historians have emphasized that the Catherinean reforms were stillborn and that the nobility's corporate bodies were inexorably bureaucratized. See Korf, *Dvorianstvo i ego soslovnoe upravlenie za stoletie 1762–1855 godov* (St. Petersburg, 1906).

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important sense the first half of the nineteenth century witnessed a fundamental process of estate formation, not breakdown, as historians have assumed. My argument represents a radical break with the traditional periodization of Russian social history, in effect moving the main dividing line from 1700 to 1800. But that kind of radical reformulation is in order if Russian social history is to be liberated from the dead weight of the state school, if social history is to be written "from below" rather than "from above."

LET US TURN NOW from terminology to the social structure of prereform Russia. At issue are two traditional propositions in the historiography—the existence of a four-estate paradigm and the predominant role of the state in creating "artificial" estates.

As a descriptive model, the four-estate paradigm is manifestly deficient. The primary juridical division in prereform Russian society was not by estates but by a different social and legal demarcation—those members of the society who were required to pay the poll tax and those who were not. For all the refinements in the *Digest of Laws* among various status groups, it was the poll tax registry that created the great chasm between privileged and unprivileged Russians. In a direct, literal sense, social policy focused not on formation of estates but on determining which group would be inscribed in the poll tax registry and which not. The tax itself was less important than inclusion in the registry, because that entailed a demeaning juridical status (including liability to corporal punishment), significant disabilities (for example, limitations on travel), and onerous burdens (in particular, recruit levies). Poll tax status also denied to its holders the unrestricted right of either social or geographical mobility.[33] Not surprisingly, poll tax inscription was at once a status and a stigma, conferred by birth, by legal punishment, and by social and economic failure; it was, naturally, shunned by the exempt.[34] Lasting until the military and poll tax reforms of 1874–87, the bifurcation created by the poll tax registry provided the basis for two profoundly separate social hierarchies. More important still, by stressing the aggregation of ranks into estates in the eighteenth century, the traditional historiography has obscured the fundamental continuity

[33] Those subject to the poll tax required permission from their responsible collective to travel or to shift to another status—a barrier that, as the history of the clergy shows, proved a major obstacle to upward mobility. See F. E. Den, "Podatnye elementy sredi dukhovenstva Rossii v XVIII v.," *Izvestiia Rossiiskoi Akademii Nauk*, 1918, no. 5: 267–92, no. 6: 413–44, no. 7: 679–708, no. 13: 1357–79, no. 14: 1517–48.

[34] The importance of the poll tax division was attested, for example, in the candid discussions of clerical reform in the 1860s. See Freeze, *Parish Clergy*, 313–14. Similarly, much of the discussion on urban reform between 1785 and 1830 focused on the status of merchants, who held temporary exemption from the poll tax so long as they had the capital to register in one of the three guilds; in the event of financial reverses, however, they dropped back into the poll tax population, with all the attendant disabilities and burdens. Quite understandably, noted the Committee of the Sixth of December 1826, merchants did all in their power to obtain formal noble status, a process that not only inflated the ranks of the nobility but also denuded the city of its most dynamic elements; *SIRIO*, 90: 364–65. It was awareness of this problem that impelled the state in 1832 to create a whole new status group of "honored citizenry" (*pochetnoe grazhdanstvo*).

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between the Muscovite and imperial social structures, for the poll tax division was at bottom a redefined version of the earlier Muscovite bifurcation of the population into service- and tax-bearing segments. Although the status of particular groups, such as sacristans and single homesteaders, changed, the eighteenth-century social structure—like the social idiom—remained closer to the system of the seventeenth century than to that of the nineteenth.

There is a still more fundamental problem with the four-estate paradigm: neither in the eighteenth nor in the nineteenth century did the *sosloviia* as conventionally identified comprise a simple hierarchy of four estates, not even in strictly legal terms. On the contrary, they formed an enormously complex congeries of social categories with numerous, distinct subcategories. The "aggregation thesis" has, without question, greatly exaggerated the degree to which social groups were consolidated into a simple set of large status categories. The *Digest of Laws* hinted at the underlying complexity, when it had to resort to the phrases "rural inhabitants" and "urban inhabitants" to designate the peasantry and townspeople, who, both legally and socially, actually comprised a variety of distinct, hereditary *sosloviia* in their own right. In the case of peasants, for example, the category of "rural inhabitant" subsumed state peasants, crown peasants, serfs, economic peasants (that is, peasants under the administration of a government office called the Economic College), and a host of other smaller hereditary groups such as single homesteaders, state peasants assigned to factory labor, possessionary (factory) serfs, and tributary (*iasachnye*) peasants. It was no doubt for this reason that, at least in some legal usage, the *sosloviia*, categories into which hereditary social groups were divided, appeared as distinct subcategories of *sostoianiia*, groups defined purely by legal status.[35] Thus, even in legal terms, the state did not establish four *sosloviia* but rather four juridical *sostoianiia*, which subsumed a real world of many different *sosloviia*.

Moreover, these groups differed not only in status but also in their defining principle and social physiognomy. Thus, some groups denoted by law and popular idiom as *sosloviia* were not even hereditary; instead, they acquired the status through personal choice, individual achievement, or in recognition of economic success.[36] Still more extraordinary were the professional groups formed in the first half of the nineteenth century, which had corporate status but were only feebly linked by heredity. Finally, while all *sosloviia* exhibited considerable variation in the economic and occupational profiles of their members, the smaller groups, for

[35] For an example of such usage, see the "Proekt dopolnitel'nogo zakona o sostoianiiakh," which divided the peasant *sostoianie* into two *sosloviia*—state and serf peasants; *SIRIO*, 90: 366. For evidence of the persisting complexity, see Preobrazhenskii's discussion in his essay "Ob evoliutsii." He also stressed the need for greater recognition of this characteristic; "Izuchenie v desiatoi piatiletke sotsial'no-ekonomicheskoi i politicheskoi istorii SSSR perioda feodalizma," *Istoriia SSSR*, 1981, no. 6: 82.

[36] This phenomenon was most apparent in the case of the merchants, who entered the guilds—with attendant privileges and corporate organization—merely by virtue of declaring a specified minimum capital. As noted above, the status was easily forfeited through economic failure and in fact had to be renewed annually. For the case of other peculiar *sosloviia*, see Korkunov's discussion of personal nobles, clergy, and honored citizens; *Russkoe gosudarstvennoe pravo*, 279, 307.

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example, Cossacks, soldiers, Jews, and other minorities, actually constituted subsocieties rather than strata.[37]

As this sketch suggests, the eighteenth and nineteenth centuries did not witness the simple aggregation of medieval *chiny* into *sosloviia* as postulated in the traditional view. To be sure, there was a tendency to conflate the innumerable social categories into larger groups.[38] That process is most evident in the case of the privileged nobility, which acquired clearer definition, common status, and a collective name.[39] But in no sense did the state "complete" this process of aggregation and unification. Before 1800 it lacked the conceptual and semantic categories for systemic social policy and thereafter not only tolerated heterogeneity within its broad social categories but also generated and codified new hereditary groups within this system. Hence, the state recognized an amorphous and shifting category of *raznochintsy* (those who did not belong to one traditional group) as well as numerous other subgroups.[40] As a perceptive observer noted in the mid-1820s, "There exist many different *sosloviia*, *sostoianiia*, and *klassy* of people in Russia.[41]

It was this social dynamic, tolerated and even abetted by the state, that no doubt produced the amorphousness, plasticity, and complexity in the *soslovie* system. In that sense historians should not exaggerate the regressiveness of the prereform social order; though superficially resembling the Western estate system, the *soslovie* structure was far more differentiated and fluid than a rigid "four-estate" paradigm would allow. Although research has long demonstrated the feasibility of mobility within the *sosloviia* (and the contradictory state policies behind this phenomenon), it is important to note as well that the *soslovie* structure itself was highly flexible,

[37] To be sure, hereditariness of the professions was encouraged, but it hardly sufficed to satisfy the need for such specialists. See, for example, L. A. Bulgakova's discussion of state policy toward surveyors, agronomists, foresters, doctors, and the like; Bulgakova, "Soslovnaia politika v oblasti obrazovaniia vo vtoroi chetverti XIX v.," *Voprosy politicheskoi istorii SSSR* (Moscow, 1974), 116–17. Analysis of economic differentiation is, of course, a commonplace in secondary works, as, for example, in the industrious efforts by Soviet historians to demonstrate stratification among townspeople and peasants. For typical examples of such analyses, see *K voprosu o pervonachal'nom nakoplenii v Rossii XVII–XVIII vv.* (Moscow, 1958); and *Goroda feodal'noi Rossii* (Moscow, 1966). For the formation of Cossacks as a distinct *soslovie*, see A. I. Pronshtein, *Zemlia donskaia v XVIII v.* (Rostov-na-Donu, 1961), 107–17, and "K istorii vozniknoveniia kazach'ikh poselenii i obrazovaniia sosloviia kazakov na Donu," in *Novoe o proshlom nashei strany* (Moscow, 1967), 158–73. On the army, see Elise Kimerling, "Soldiers' Children, 1719–1856: A Study of Social Engineering in Imperial Russia," *Forschungen zur osteuropäischen Geschichte*, 30 (1982): 61–136. On the status of the Jews, see Michael Stanislawski, *Tsar Nicholas I and the Jews: The Transformation of Jewish Society in Russia, 1825–1855* (Philadelphia, Pa., 1983), 8, 14, 47–48, 125, 184.

[38] The general process of conflation began, significantly, in the seventeenth century and represents an attempt at simplification of social categories in fiscal-legal practice. See, for example, V. M. Vazhinskii, "Vvedenie podushnogo oblozheniia na iuge Rossii v 90–kh godakh XVII v.," *Voprosy sotsial'no-ekonomicheskoi istorii Rossii XVIII–XIX vv.*, in *Izvestiia Voronezhskogo gosudarstvennogo universiteta*, 1975, no. 127: 88–103.

[39] For the appearance of *shliakhetstvo* in 1712 and its displacement by *dvorianstvo* after mid-century as collective terms for "nobility," see A. Romanovich-Slavatinskii, *Dvorianstvo v Rossii ot nachala XVIII veka do otmeny krepostnogo prava* (St. Petersburg, 1870), 503–04.

[40] On the complex development of the *raznochintsy*, see Christopher Becker, "*Raznochintsy*: The Development of the Word and the Concept," *American Slavic and East European Review*, 18 (1959): 63–74; I. B. Sidorova, "Polozhenie raznochintsev v russkom obshchestve (XVIII–pervaia polovina XIX v.)," (Kandidatskaia dissertatsiia, Kazan State University, 1982); and G. N. Vul'fson, "Poniatie '*raznochinets*' v XVIII–pervoi polovine XIX v.," in *Ocherki istorii narodov Povolzh'ia i Priural'ia*, fasc. 1 (Kazan, 1967): 107–24.

[41] P. Pestel', "Russkaia pravda," reprinted in *Vosstanie dekabristov*, 7 (1958): 150.

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enabling the creation of new categories to satisfy the various requirements of state and society.[42] Expressed more baldly, the *soslovie* structure proved adaptable to the exigencies of social and economic development; a multivariate structure permitted specialization and occupational professionalization, yet within a formal system of hereditary estates. The technical and professional groups illustrate this adaptability. Hence, the first half of the nineteenth century represented not only a terminological breakthrough but also a peculiar "modernization" of *sosloviia* into more specialized occupational status groups. This process was doubtless less dynamic than a class society but was also far less rigid and inflexible than has been assumed.

A second traditional argument—that Russian *sosloviia* were artificial creations of the state and doomed to impotence—also requires qualification. The formation of specific categories did not, first of all, rest solely with the state; however important its policies might have been in legitimizing or reinforcing such social orders, the *ancien régime* with its notoriously "underdeveloped bureaucracy" was incapable of engineering, legislating, and managing a social system.[43] More important, particular social groups—at least those with any semblance of legal or economic privilege—sought to reinforce their separateness as distinct social categories. In the case of the clerical estate, for example, the parish clergy was a direct and important agent in establishing social exclusion, as it endeavored to conserve the limited number of ecclesiastical positions for its own progeny.[44] Similar processes occurred with other groups, whether on grounds of economic privilege (merchant demands for a monopoly on commerce) or of religious and national autonomy (Jewish separatism). Significantly, it was often the government—conventionally depicted as socially conservative and the main fabricator of the *soslovie* system—that intervened to override such particularism and to loosen the social order. This was, after all, the net effect of its ennoblement of commoners through the Table of Ranks, its measures to remove surplus clerical offspring, and its decision to recognize the commercial rights of peasants and squires. Finally, in many cases the state simply conferred legal recognition on what already existed, for example, on traditional subsocieties, such as Jews and Cossacks, and on new social categories, such as "trading peasants." In the case of the trading peasants, both the

[42] Suggestive of the state's willingness to tolerate some mobility were the deliberations of the Committee of the Sixth of December 1826, which found it desirable that "the path from one *soslovie* to another be barred to none, but that this transfer all take place within the limits set by law." Significantly, the committee here used *soslovie* as a subcategory of *sostoianie*, implying that movement might occur within the four main *sostoianiia* but not between them; *SIRIO*, 90: 367. For a good discussion of these contradictory strains in state policy, see V. R. Leikina-Svirskaia, "Formirovanie raznochinskoi intelligentsii v Rossii," *Istoriia SSSR*, 1958, no. 1: 83–104. It was clearly the sensitivity to this fluidity that provoked rising unease in high circles in the late Nikolaevan era, manifested in the more restrictive policies of the Ministry of Education and in such internal memoranda as that by A. Kaminskii; "O narodnom prosveshchenii i o glavnykh sosloviiakh v Rossii," *Russkaia starina*, 1903, no. 115: 177–89.

[43] For an early, cogent statement on the "underinstitutionalization" of prereform state administration, see S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870* (Princeton, N.J., 1972), 44–50.

[44] G. L. Freeze, *The Russian Levites: Parish Clergy in the Eighteenth Century* (Cambridge, Mass., 1977), chap. 6.

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phenomenon and the term antedated the Kankrin reform of 1824, which served not to invent a new status group but only to define its proper place in the established order.[45]

It is also necessary to modify the corollary that *sosloviia* were "weak" artifacts without authority, cohesion, or self-awareness, created by the state at its own whim. That notion, encouraged by Tsar Nicholas I's criticism of Catherinean corporate bodies (and his proclivity to reassert bureaucratic absolutism), found its way into the state school of historiography and ever since has been a salient feature of the social history of prerevolutionary Russia. But, as became clear during the Great Reforms, the disinclination of social groups to collaborate with the state voluntarily was no measure of their capacity to function as corporate bodies; rather, it was a means of evasion and noncompliance.[46] But even before that confrontation between state and *sosloviia* in the 1860s, the structure of *soslovie* relations with the state can hardly be reduced to a simple matter of "bureaucratic stifling of the living forces of the nation"—the politically charged notion that originated in prerevolutionary liberal historiography and that has since prevailed. As has often been noted, the state routinely sought to establish close ties between a particular group and a particular state bureau. But the interaction between state bureau and estate worked in both directions: the former not only exercised influence over the latter but also articulated the group's special interests. The Russian *soslovie*, in contrast to Western estates, may have lacked autonomous social organization, but its close ties to the state provided a legal and serviceable substitute—indeed, one that meshed well with the bureaucratic structure of the traditional regime. The responsible bureau, after all, had a vested interest in the prosperity and well-being of its charges, and, in the case of the nobles and clergy, it even had direct kinship ties. It is hardly surprising that in prereform Russia, and still more saliently in postreform Russia, particular ministries tended to articulate the interests of a subordinate or closely associated group—the Ministry of War spoke for the soldiers, the church for the clergy, the Ministry of Interior for nobles, and the Ministry of Finance for townspeople. If society in Russia was to an unusual degree "bureaucratized" by the state, so too, conversely, was the state "socialized" by the interpenetration of bureau and estate.

A THIRD FUNDAMENTAL POSTULATE in the traditional historiography—that, beginning with the Great Reforms of the 1860s, the *sosloviia* were gradually transformed into modern classes—is also in need of revision. The *sosloviia* in fact proved extraordinarily hardy and lasted until the end of the *ancien régime*. To be sure, they

[45] For discussions of this important interstitial category, see Hildermeier, "Bürgertum und Stadt in Russland"; and D. Morrison, "'Trading Peasants' and Urbanization in Eighteenth-Century Russia: The Central Industrial Region" (Ph.D. dissertation, Columbia University, 1981). There is also a substantial corpus of Soviet literature. See, for example, G. L. Vartanov, "Kupechestvo i torguiushchee krest'ianstvo tsentral'noi Rossii vo vtoroi polovine XVIII v.," *Uchenye zapiski Leningradskogo gosudarstvennogo pedagogicheskogo instituta im. A. I. Gertsena*, 229 (1962): 161–92.

[46] On the obvious case of the gentry, see Daniel Field, *The End of Serfdom* (Cambridge, 1976). To compare the clergy's opposition to ecclesiastical reform in the 1860s and 1870s, see Freeze, *Parish Clergy*, chaps. 7–8.

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were not wholly immune to the substantial social and economic changes that intruded into imperial Russia, at first slowly, then with increasing force and effect. Yet, in law and social consciousness, the *soslovie* structure remained astonishingly resilient, providing the primary means of collective identity and forming the bedrock of social stratification in late imperial Russia. That has been an important discovery in recent research on particular groups,[47] but a full analysis of the development of the *sosloviia*, attentive to the dynamics of state policy and social structure, has yet to be made. At most, historians have sensed a certain "complexity" in the social order but without relating this to fundamental structural problems in the social stratification of postreform Russia.[48] It is important to recognize the powerful forces that gave such extraordinary tenacity and vitality to the "antiquated" *soslovie* system in a society already on the path of industrialization.

This system had roots that went deeper than the *Digest of Laws*. For example, hereditariness and a strong tendency toward endogamy had a firm basis not only in law but also in property relations, custom, and prestige. Even when the legal underpinnings of the social structure began to dissolve, invisible social bonds helped sustain the system of separate orders. This characteristic separateness of the estate system was further reinforced by significant cultural differences—in way of life, values, religious practice, and education. Indeed, as the recurring talk of "casteness" implies, culture formed an important barrier between the various estates. Especially in the period before 1890, when public education remained ineffective and abysmally stunted, the school as yet did little to erode the traditional differences in group subcultures and to provide a common cultural identity.

Moreover, Russian autocracy sought to reinforce, not weaken, the *soslovie* order, even if historians have customarily argued that the Great Reforms of the 1860s represented a major assault on the system. But, given what we know about Alexander II and leading "reformers," it is hard to imagine that they intended to reconstruct the whole social system.[49] To be sure, some reforms—in local self-government (the zemstvo), city administration, taxation, and universal military conscription—did transcend *soslovie* boundaries, but the objective was to maximize resources and efficiency, not erase the separateness and isolation of individual groups. In the prerevolutionary idiom, the Great Reforms were "all-estate" (*vsesoslovnyi*), not "non-estate" (*bessoslovnyi*)—that is, they included all estates but deliberately preserved their legal distinctiveness and social separateness.[50] The state's policy was most apparent in reform of the administrative and

[47] See, for example, Freeze, *Parish Clergy*, chaps. 8–10; Alfred J. Rieber, *Merchants and Entrepreneurs in Imperial Russia* (Chapel Hill, N.C., 1982); Robert J. McNeal, *Tsar and Cossack, 1855–1914* (London, 1984); and Hamburg, *Politics of the Russian Nobility*.

[48] For a general statement, indicating unease over the complexity and confusion in social stratification, see Terence Emmons, *The Formation of Political Parties and the First National Elections in Russia* (Cambridge, Mass. 1983), 1.

[49] On Alexander II, see Iu. I. Gerasimova, "Krizis pravitel'stvennoi politiki v gody revoliutsionnoi situatsii i Aleksandr II," in *Revoliutsionnaia situatsiia v Rossii v 1859–1861 gg.*, 2 (Moscow, 1962): 93–106. For historiography on the Great Reforms and estate policy, see Korkunov, *Russkoe gosudarstvennoe pravo*, 275; Emmons, *Formation of Political Parties*, 1; and Blum, "Russia," 79–80.

[50] This important distinction, which sharpened steadily in the 1860s and 1870s, was concisely formulated in 1873 by a key official, P. A. Shuvalov. "I would express [my program] in two words:

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social infrastructure, which preserved the fundamental segregation of social categories, and in all reforms affecting the peasantry, which was treated as a separate subsociety and given its own courts, customary law, and system of administration. Significantly, even the Great Reforms that violated *soslovie* interests and integrity had the effect of hardening, not eroding, group identity and cohesiveness. Thus, in the case of the Orthodox clergy, where the state attempted to diminish what was widely regarded as a debilitating hereditariness, its reforms aimed only at slow, incremental change but aroused fierce opposition among the ostensible beneficiary, the parish clergy.[51] Similar dissatisfaction pervaded the landed nobility, which felt profoundly aggrieved by the process and terms of serf emancipation. Indeed, the very fact that prominent segments of the nobility opposed the regime, using their corporate organization to articulate protests and demands for a greater role in the political system, made the regime more determined than ever to preserve the "separateness" and stability of the traditional estates.

Moreover, the state's commitment to the *soslovie* system became increasingly intense—and ideological—after 1870. A dogmatic reassertion of the importance of the estates came, in the first instance, from the state itself. Disappointed in the results of the reforms of the 1860s and alarmed by the upsurge of political and social unrest, the state sought to rebuild the social foundations that had existed in the prereform period. Further impetus came from the conservative gentry, who propagandized the *soslovie* system as a fundamental component of autocracy in a calculated effort to obtain special interest legislation.[52] By far the most notable statement of this idea appeared in an essay, *The Contemporary Condition of Russia and the "Soslovie" Question*, by a provincial nobleman, Aleksei D. Pazukhin, who argued that the Great Reforms had weakened the empire's *soslovie* system and that the state must take immediate and firm countermeasures.[53] The result was a deluge of legislation—to bolster the economic status of individual groups (for example, separate banks for the peasantry and nobility), to strengthen the system of voting by estates in local government elections (the zemstvo counterreform of 1890), to restrict the influx of upstarts into administration, and to direct education to serve individual groups.[54]

Yet, at the same time that the state attempted to bolster the *soslovie* system, social

vsesoslovnost', but not *bessoslovnost'*—that is, a harmonious inclusion of *sosloviia* in the affairs of general state interest, but not at all through their absorption into a single, undifferentiated popular mass"; Shuvalov, as quoted in I. V. Orzhekhovskii, "Tret'e otdelenie," *Voprosy istorii*, 1972, no. 2: 114.

[51] Freeze, *Parish Clergy*, 308–15, 319–29, 383–88.

[52] For a case study showing the reassertion of *soslovnost'*, see V. G. Chernukha, *Vnutrenniaia politika tsarizma s serediny 50–kh–nachala 80–kh gg. XIX v.* (Leningrad, 1978), 199–243. On the gentry, see Hamburg, *Politics of the Russian Nobility*, 107–10, 122; and A. P. Korelin, *Dvorianstvo v poreformennoi Rossii, 1861–1904 gg.* (Moscow, 1979), 270–84.

[53] *Sovremennoe sostoianie Rossii i soslovnyi vopros* (St. Petersburg, 1885). For a discussion of Pazukhin and his role in the counterreforms of the late 1880s, see T. Taranovsky, "The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III, 1881–1894" (Ph.D. dissertation, Harvard University, 1976), 373–441.

[54] For a good survey of *soslovie* legislation for the nobility, see Thomas S. Hause, "State and Gentry in Russia, 1861–1917" (Ph.D. dissertation, Stanford University, 1973), 80–163.

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stratification and identity were gradually being modified. New groups formed, and distinctions among groups became blurred by social and economic changes engendered by the state's own industrialization policies. Not that such changes were previously unknown. But the development that came in the late nineteenth century had a different scale and, more important, clashed with the state's dogmatic determination to uphold the earlier system of estates and with its refusal to accord legal recognition and legal status to the new groups.

Workers comprised one important new category. Their ranks swelled significantly after the onset of rapid industrialization in 1885. Despite their numbers, volatility, and political visibility, the state refused to accord workers legal recognition as a "class," instead regarding them as a special subgroup of the regular peasantry.[55] But the workers plainly were different. Even if not the "self-conscious class" postulated in Soviet historiography, workers did have a markedly different physiognomy—caused by the social psychology of their geographic concentration, the availability of established models among identifiable peers in the West, and the very refusal of the state to grant separate legal status. The phrase "working class" (*rabochii klass*) appeared early in the postreform period, and contemporaries were quick to discern the uniqueness of this new and important social category.[56]

Still more anomalous were the professions. Though found in prereform Russia, the group proliferated rapidly after the 1860s, not only increasing in aggregate size but also diversifying in categories and status.[57] Some of the professions, especially those (such as doctors) who enjoyed official recognition as a professional estate (*Berufsstand*) in the prereform era, developed formal organizations and had a distinct legal status. With few exceptions (most notably, lawyers), other new professions and semiprofessions lacked a clear standing in law and had few, if any, privileges.[58] This indefinite or ambiguous legal status was important, for above all

[55] Classic evidence of state attitudes appeared in a government circular of 1895, which flatly asserted that "Russia, fortunately, does not have a working class in that sense and significance as in the West." See I. K. Ozerov, *Politika po rabochemu voprosu v Rossii za poslednie gody* (St. Petersburg, 1906), 24–25.

[56] Curiously, the *soslovie* idiom imposed itself even in the case of workers, for informed and sympathetic observers unwittingly referred to the "worker *soslovie*." See N. I. Pirumova, "Zemskaia intelligentsiia v 70–e–80–e gody XIX v.," *Istoricheskie zapiski*, 1981, no. 106: 137.

[57] For a general account, see V. R. Leikina-Svirskaia, *Intelligentsiia v Rossii vo vtoroi polovine XIX v.* (Moscow, 1971). For interesting case studies of particular professions and semiprofessions—doctors, penologists, teachers, statisticians, and surveyors, see Frieden, *Russian Physicians*; Bruce Adams, "Criminology, Penology, and Prison Administration in Russia, 1863–1917" (Ph.D. dissertation, University of Maryland, 1981); Scott J. Seregny, "Professionalism and Political Activism: The Russian Teachers' Movement, 1864–1908" (Ph.D. dissertation, University of Michigan, 1982); B. B. Veselovskii, *Istoriia zemstva za sorok let*, 4 vols. (St. Petersburg, 1909–11), 3: 469–71; Bernhard Schalhorn, "Verwaltung und technische Intelligenz: Das Ausbildungswesen für den zivilen Vermessungsdienst in Russland," paper presented to the symposium, "Die russichen Intellektuellen und die Intelligenzija um die Jahrhundertwende," held in Berlin, February 8–10, 1978.

[58] An important forerunner of professional organization was the St. Petersburg Society of Russian Doctors, founded in 1836 and followed by many others at the provincial level by the time of the Great Reforms. For a survey of twenty such societies, see "Gubernskie meditsinskie obshchestva," *Meditsinskii vestnik*, 1864, no. 48: 457. On the development of the national medical organization, the Pirogov Society, which held its first congress in 1885, see Frieden, *Russian Physicians*. Before the 1860s Russia had no recognized profession of private lawyers with professional training and organization. As the State Council aptly observed in 1857, "We do not have a *soslovie* of lawyers." But the judicial reform of 1864 established a new private legal profession and specifically provided for its corporate organization. I. V. Gessen and M. N. Gernet, *Istoriia russkoi advokatury*, 1 (Moscow, 1914): esp. 37.

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it precluded both autonomous organization and the right to hold periodic congresses to discuss common professional interests. It was only with great difficulty that the new professions obtained any form of official recognition; even then their right to organize was severely restricted, for example, both in the groups' ability to judge the competence of their members and in the geographical scope of their membership. Recruited from many groups, provided with semispecialized education, denied autonomous professional organization, the new professions did not fit into the existing social hierarchy of corporate *sosloviia*. Lack of corporate status was particularly apparent in the case of the newer professions—teachers, statisticians, agronomists, foresters—which lacked legal recognition in prereform Russia and found employment after 1864 primarily in the zemstvos, the organs of self-government that existed separately from the regular bureaucratic order. As a result, these groups remained outside the traditional *soslovie* matrix and grew increasingly discontented over their lack of organization, rights, and perquisites. Quite understandably, they required a new terminology for self-description—first, the more general term "intelligentsia" and, later, the more specific label of "Third Element."[59] In social idiom as well as in law, these new strata generated a descriptive terminology wholly outside the *soslovie* system.

This supra-estate status of the intelligentsia became integral to its identity and political strategy. The professions boasted an "all-estate" membership and, as a result, developed aspirations and claims to articulate the interests of all social groups.[60] Indeed, the *soslovie* system impinged deeply on the social thought and language of the intelligentsia. Although "class" language gradually gained currency, especially after the appearance of Marxism, use of the term *soslovie*—and concern about the inequities that the system entailed—was especially prevalent among nonrevolutionary members of educated, liberal groups. The state's attempt to reinforce the *soslovie* system only intensified opposition to it. Increasingly critical of the civil inequality and segregation inherent in the estates, educated members of society demanded such reforms as the creation of an all-estate system of local government at the cantonal level (*melkaia zemskaia edinitsa*).[61] Such supra-estate ideas were indeed central to most political programs but, above all, to those emanating from the liberal gentry and intelligentsia on the eve of 1905.[62]

[59] The term "intelligentsia" appeared in the 1860s and, along with the term *obshchestvo* ("society"), came to denote various strata of educated society that were not part of the bureaucracy and autocracy. The term "Third Element," first coined in 1899, quickly gained popularity and was applied to the professional employees of the zemstvos, who, together with the elected zemstvo officials (the "second element") and the bureaucracy ("the first element"), formed the three main political forces in Russian state and society.

[60] The phrase *vsesoslovnaia intelligentsiia* abounds. See, for example, the statements by the moderate noble D. N. Shipov; I. P. Belokonskii, "Zemskoe dvizhenie do obrazovaniia partii narodnoi svobody," *Byloe*, 1907, no. 9: 235.

[61] For a summary, see Veselovskii, *Istoriia zemstva*, 3: 656–65.

[62] In a typical programmatic statement in the underground liberal newspaper *Osvobozhdenie*, liberal leaders called for "the equalization of peasants in rights with all other *sosloviia*, but also the abolition of noble privilege in the sphere of administration, in zemstvo representation, and in *soslovie* limitations on landownership"; "Ot russkikh konstitutsionalistov," *Osvobozhdenie*, June 18, 1902, p. 9. This goal was later basic to the program of the liberal Constitutional-Democratic ("Kadet") party. See Konstitutsionno-demokraticheskaia partiia, *Postanovleniia III s"ezda 21–25 aprelia 1906* (St. Petersburg, 1906), 3.

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Significantly, anti-estate notions provided not only the program but also the strategy of liberal and even left oppositionists in their struggle against autocracy before 1905. That tendency was most salient in the case of Russian liberalism. Organizationally centered in the Union of Liberation, this movement aspired to remain "above" the interests of any particular group, to "express solely the non-estate (*bessoslovnoe*) public opinion and rely upon it."[63]

As the examples of workers and intelligentsia suggest, the social structure had grown increasingly complex, no longer conforming to definitions recognized in law. The discrepancy between formal stratification and social reality was most acute among the most political, most self-conscious groups—the educated professions and the turbulent working class. At the same time, the *soslovie* system remained entrenched in the outlook of traditional groups, especially the nobility and still more the peasantry.[64] Indeed, since many noblemen had nonagrarian professional careers, the peasantry was by far the most rigid, separate, and distinctive component in the whole social order, in good measure because of their strict segregation from the rest of the administrative, judicial, and economic system. Other *sosloviia* remained separate and distinct but evinced a growing disparity between social origin and social status, especially as offspring departed the traditional estate to find some new form of employment. Although the state still reaffirmed the basic *soslovie* system (reissuing, for example, the *Digest of Laws* in 1899 without change), the social structure now included significant but unrecognized status groups. Furthermore, there is evidence that social identities were becoming amorphous and confused. The legal historian Korkunov argued that, by the turn of the century, most of his contemporaries had no clear idea of their estate status, and, though that is plainly an exaggeration, it correctly indicates some significant processes of social change.[65] Ambiguity about social identity, at least as defined by the *sosloviia*, pervaded popular literature and, more significantly, efforts at self-description by various social groups.[66] As data compiled in 1905 suggest, most members of society still made an estate their primary reference group, but many others introduced ethnic background, occupation, or property ownership as their principal social identity.[67] The complexity of the social order impinged

[63] "Ot russkikh konstitutsionalistov," *Osvobozhdenie*, June 18, 1902, p. 7.

[64] This observation was made often by contemporaries. See, for example, comments by A. S. Stishinskii; *Petergofskoe soveshchanie o proekte Gosudarstvennoi dumy: Sekretnye protokoly* (Berlin, n.d.), 103.

[65] Korkunov, *Russkoe gosudarstvennoe pravo*, 275. The state still issued documents and assembled data primarily on the basis of *soslovie* categories, not occupation—as in its all-important census of 1897. Similarly, individuals still often described their status in terms of traditional *soslovie* categories, as in the authorized biographies of Duma members. See *Al'bom portretov chlenov Gosudarstvennoi dumy pervogo prizyva: Portrety, kratkie biografii i kharakteristiki deputatov* (St. Petersburg, 1906). No doubt, it is because of such documents that historians have continued to assemble data according to *soslovie* categories, even for the final years of the *ancien régime*. On the composition of various parties by *soslovie* categories, see, for example, Robert Edelman, "The Election to the Third Duma: The Roots of the Nationalist Party," in Haimson, *Politics of Rural Russia*, 100. Use of *soslovie* categories, interestingly enough, routinely pervades even Soviet historiography, which, at least in theory, is analyzing a society composed of classes, not estates, in the early twentieth century. See, for example, V. S. Diakin, *Samoderzhavie, burzhuaziia i dvorianstvo v 1907–11 gg.* (Leningrad, 1978), 9.

[66] For the theme of success and attendant social mobility, see Jeffrey Brooks, *When Russia Learned to Read* (Princeton, N.J., 1985), 269–94.

[67] See, in particular, Emmons, *Formation of Political Parties*, 173–74. In the data assembled by

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directly on the government in 1905, when it explicitly acknowledged the situation and employed a variety of criteria—property, *soslovie*, and occupation—to broaden suffrage after the October Manifesto.[68]

The revolution of 1905 unleashed broad-based opposition to the *soslovie* system but did not end in its abolition. On the one hand, the revolution intensified demands for the establishment of civil equality and, especially, for the inclusion of the peasantry in the body politic. Strikingly, such demands appeared not only in the programmatic statements of the liberal parties but also in declarations by more articulate peasants, who demanded "the equality of all citizens" and "the abolition of all *sosloviia* and ranks."[69] Opposition to the *soslovie* system among the unprivileged is understandable, but their attitude was widely shared by the propertied and moderate, including the right-centrist Octobrist party. As one Octobrist explained, "The *sosloviia* have outlived their time," at least politically, for "the recognition of equality exists among the entire people."[70] The Duma, after its convocation in 1906, also pressed for the abolition of estates, even in its first declaration to the throne at the outset of its operation.[71] The issue percolated throughout debates in the First and Second Dumas in 1906–07, and, on June 2, 1907—the day before the coup d'état of June 3—a liberal faction filed a proposal for legal abolishment of the *soslovie* system.[72]

For all the clamor against the estates, especially from moderate and radical parties as well as articulate members of the unprivileged, the formal structure and social identity of the *sosloviia* did not disintegrate in the midst of the revolutionary turmoil. That applies, above all, to the privileged groups—the gentry, clergy, and

Emmons, various categories of profession, class, and *soslovie* are intermingled in respondants' own descriptions of their social status (*zvanie*, which could refer to either occupation or social estate).

[68] Even before the circumspect Bulygin proposal drafted in early 1905, the Council of Ministers aptly noted the changes overtaking the formal *soslovie* order. "Although [Russia] is, in law, based on the principles of estate organization, the economic process common to all peoples together with a whole series of state reforms enacted in recent decades have already long shaken and to a great extent even destroyed the *soslovie* divisions delineated by law"; *Materialy po uchrezhdeniiu Gosudarstvennoi dumy* (St. Petersburg, 1905), 129. As the Council of Ministers discovered in late 1905, when it sought to expand the Bulygin electoral law to include the intelligentsia and workers, it was no simple task to devise a variety of property, *soslovie*, and occupational criteria that—short of universal franchise—would enable the government to achieve nearly full franchise yet control the proportions and interaction of the various categories. The council was particularly concerned that the electoral process include those groups that "are usually considered the most subject to unrest (people in the professions, workers, etc.)" and that failed to meet either the traditional *soslovie* or the newer property criteria. See "Memoriia Soveta Ministrov o sposobakh osushchestvleniia Vysochaishikh predukazanii, vozveshchennykh v punkte 2–m Manifesta 17 Oktiabria 1905 g.," Tsentral'nyi gosudarstvennyi istoricheskii archiv SSSR, Leningrad, fond 1276 (Sovet Ministrov), opis' 1, delo 39, listy 29–39.

[69] For collective peasant declarations, see S. Dubrovskii and B. Grave, *1905: Agrarnoe dvizhenie v 1905–1907 gg.*, 1 (Moscow, 1925): 8, 39, 85; and F. I. Kalinychev, *Gosudarstvennaia duma v Rossii* (Moscow, 1957), 161–62, 165, 230, 267–69.

[70] N. N. Opochinin, as quoted in Gosudarstvennaia duma, vtoroi sozyv, *Stenograficheskii otchet*, 2 (St. Petersburg, 1907): 109.

[71] Gosudarstvennaia duma, pervyi sozyv, *Stenograficheskii otchet*, 2 vols. (St. Petersburg, 1906), 1: 175.

[72] One bill stated, "For the full development of the principle of legal equality and individual freedom in the state, it is necessary to abolish the *soslovie* system, that is, the division of the population into groups solely according to the principle of common origin, as a result of which members of *soslovie* corporations possess political and other rights (established by law and transmitted by heredity), which are unequal for various groups." For the full proposal, see Gosudarstvennaia duma, vtoroi sozyv, *Zakonodatel'nye materialy* (St. Petersburg, 1907), 265.

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industrial and commercial elites—and to such traditional subgroups as the Cossacks. Most supportive of the *soslovie* system were the nobles, who founded the Council of United Nobility with the explicit goal of defending the existing order in general and their own privileged status in particular. The same applies to the clergy and merchants and industrialists, who exploited the turmoil of revolution to form corporate *soslovie* organizations and who, for the most part, still observed traditional distinctions. And the Duma debates bear witness that the sense of differences among estates remained deeply embedded in the consciousness of the delegates—from nobles to peasants—who still analyzed social issues more often in the idiom of *soslovie* than of class.[73]

Although the revolution of 1905–07 failed to transform estates into classes, it did serve to politicize the conception of *soslovie*, as the government and its adversaries reevaluated the political "reliability" of particular *sosloviia*. This change was most striking in the case of the state, hitherto committed to the defense of the whole *soslovie* system. The revolution of 1905 shattered that commitment; opposition among educated elites, even the nobility, impelled the state to seek new principles and policies to identify and institutionalize secure social foundations for autocracy. This shift was already apparent by the summer of 1905, as the state struggled to fulfill its promise to allow some kind of popular franchise and to convoke a consultative assembly. As the secret discussions at Peterhof reveal, the *soslovie* system was not without defenders, but they were a minority. The franchise, it was decided, would be based chiefly on ownership of property, not membership in a *soslovie*. This fundamental shift, as one member of the Peterhof conference explained, was in part necessitated by the complexity of the contemporary social order. "The Council of Ministers decided to reject elections on a *soslovie* basis for the reason that not all parts of the population of the Russian empire fit within the *soslovie* framework."[74] Although the state was partly concerned about the complexity of the social order, it was even more disturbed by the complexity within individual *sosloviia*. Since even the noble estate included not only conservative gentry but also liberal professionals and revolutionary Polish aristocrats, the state was forced to go beyond the criterion of *soslovie* membership in its all-important electoral system. It was only in the case of the peasantry, who seemed such a staunch supporter of autocracy, that the government made explicit allowance for special representation as a *soslovie*.[75]

But peasant radicalism shattered these illusions and drove the regime to act ever more consistently in making class—economically defined—the foundation of its new order. A pivotal figure in this shift was Sergei Iu. Witte, who had first encountered and opposed ideological defense of the *soslovie* system in the 1890s,

[73] For the addresses in the first session of the Duma in 1906 by moderate gentry, peasants, and Cossacks, see Gosudarstvennaia duma, pervyi sozyv, *Stenograficheskii otchet*, 1: 79, 87, 130; 2: 1101.

[74] *Petergofskoe soveshchanie*, 110–11.

[75] On the electoral system and the state deliberations that lay behind it, see Gilbert S. Doctorow, "The Introduction of Parliamentary Institutions in Russia during the Revolution of 1905–1907" (Ph.D. dissertation, Columbia University, 1975), chaps. 1–4; and Andrew V. Verner, "Nicholas II and the Role of the Autocrat during the First Russian Revolution" (Ph.D. dissertation, Columbia University, 1985), chaps. 5–6.

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when the gentry endeavored to exploit the regime's earlier commitment to the *soslovie* principle for their own material benefit and to stall his industrialization program. But Witte's hostility to the system steadily deepened in the critical years preceding 1905, as he expanded his conception of the prerequisites for economic growth to include not only the influx of foreign capital but also changes in the cultural and social system.[76] During his brief tenure as prime minister (October 1905–April 1906), Witte vigorously attacked the juridical barriers dividing the various estates—for instance, in his plans to alter communal landholding and to construct an all-estate administration and judiciary at the lowest levels. His eventual successor, Petr A. Stolypin, systematically expanded these policies.[77] Most famous was his agrarian program, which, by converting and consolidating communal land into private property, aspired to "bank on the propertied," not the *sosloviia*. Still more characteristic of the new social policy was the decree of October 5, 1906, that flagrantly contravened the *soslovie* system: "All Russian citizens, regardless of family origin, have the same rights with regard to government service as those rights previously accorded the nobility."[78] Of particular importance were his plans to reform local government and justice, which in effect sought to eliminate the nobility's privileged status and, especially, the inferior, segregated condition of the peasantry. Stolypin's goal, it should be stressed, was not to abolish the *sosloviia* but to make institutions socially inclusive, to extend the all-estate principle to the administrative infrastructure, and thereby to build firmer foundations for autocracy.

The all-estate, and in large measure non-estate, policy had much to commend it. It was politically attractive; moderates as well as liberals, Octobrists as well as Kadets, favored the formal legal equality of all groups. For Stolypin and many others, the real question was the peasantry, and it was widely believed that the only way to solve the "peasant problem"—the backwardness of the peasants, their dire economic straits, and their consequent political volatility—was to make them into full-fledged members of the body politic. Moreover, it became increasingly evident that neither social identities nor occupational structures corresponded in any significant way to the traditional estate categories. Even the most zealous defenders of the old order conceded that Russia now had "a mixed system of estates and classes."[79] Although that phenomenon may have long been apparent in the rise of such new categories as workers and semiprofessionals, after 1905 observers recognized an erosion of the *soslovie* system, at least partly, among the peasantry,

[76] The development in Witte's thinking is strikingly apparent in two memoranda from 1900 and 1904. See his "O polozhenii nashei promyshlennosti," in "Dokladnaia zapiska Vitte Nikolaiu II," *Istorik-Marksist*, 1935, no. 2–3 (42–43): 130–39, and *Zapiska po krest'ianskomu delu* (St. Petersburg, 1904).

[77] For a good account of Witte's aims, see his statement to the Council of Ministers in January 1906, as quoted in Geoffrey Hoskings, *Russia's Constitutional Experiment: Government and Duma, 1907–1914* (Cambridge, 1973), 18; and Weissman, *Reform in Tsarist Russia*, 101–04. On Stolypin, see Diakin, *Samoderzhavie, burzhuaziia i dvorianstvo*, 38–39; and Hoskings, *Russia's Constitutional Experiment*, 155–56.

[78] *Polnoe sobranie zakonov rossiiskoi imperii*, 3d ser., 28 (St. Petersburg, 1907), no. 28392, art. 1. For details, see Weissman, *Reform in Tsarist Russia*, 124–47; and Diakin, *Samoderzhavie, burzhuaziia i dvorianstvo*, 38–44.

[79] Speech by F. D. Samarin at the Council of United Nobility, *Trudy IV-ogo s"ezda upolnomochennykh ot dvorianskikh obshchestv* (St. Petersburg, 1908), 18, and 99–102.

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nobility, and clergy. Perhaps the most significant testimony to the new social reality came from the Council of United Nobility, the archconservative organization fervently committed to the preservation of *sosloviia* in general and the nobility in particular. Yet even this group, at its congress in 1907, conceded that society now consisted of "estate-groups" (*soslovnye gruppy*), not genuine *sosloviia*.[80] A committee report to the council in 1906 explained: "Estate bonds have been weakened and gradually replaced by a natural connection between people who have a similar way of life that is based upon property, occupation, education, etc."[81]

Although these bonds had weakened, they had not dissolved, and most among the privileged strata believed that the *soslovie* system should remain. One archconservative wrote in 1906 that Russia's survival depended on reinvigoration of the *soslovie* system. "In general, I am coming to see that the right answer for the state and people to revolution is *soslovnost'* ["estate-ness"]. . . . Orthodoxy, Autocracy, Nationality, *Soslovnost'*—these are the [real] foundations."[82] Both within the zemstvos and the Council of United Nobility, the gentry vigorously opposed Stolypin's all-estate reforms, stood resolutely in defense of the *soslovie* system, and warned against "the complete abolition of the *soslovie* principle."[83] As the records of the council's congresses show, its members were not blind to the erosion in the estate system but believed that, at least in rural Russia, the *sosloviia* still provided the primary form of social identification. Hence, the council, like conservative factions in the Duma and State Council, adamantly opposed various parts of Stolypin's reform program, including his proposals for an all-estate cantonal administration and court. It was indicative of the crisis brewing in the interrevolutionary years that the autocracy found support neither in remnants of past *sosloviia* nor in the roots of classes still in the process of development.

Not unlike eighteenth-century France, Russia developed a polymorphic social structure, with competing social hierarchies based not merely on legal status but also on a host of other characteristics.[84] The point here is not whether prerevolutionary Russian society is conceived as "neither classes nor estates" or as "estates and classes" (a formula perhaps more consonant with the tangled complexities of late imperial Russia) but that social identities remained in a high degree of ambiguity and flux, oscillating between legal estate, economic status, and occupation.[85] Although partly dismantled by the events and accommodations of the 1905–07 revolution, the *soslovie* system remained largely intact in consciousness and law, even as the *sosloviia* were reconceptualized as *kul'turno-bytovye gruppy*—that is, social groups defined by their subculture and peculiar life styles.[86]

[80] See *Trudy III-ego s"ezda upolnomochennykh ot dvorianskikh obshchestv* (St. Petersburg, 1907), 78–79, 301.

[81] *Doklad ob izbiratel'nom zakone* (St. Petersburg, 1906).

[82] "Dnevnik Borisa Nikol'skogo," *Krasnyi arkhiv*, 1934, no. 63: 66.

[83] *Trudy IV-ogo s"ezda*, 67, 88, 267. Also see "Polozheniia vyrabotannye s"ezdom 'Kruzhka Dvorian' 22–25 aprelia," in *Kruzhok dvorian vernykh prisiage: Otchet s"ezda 22–25 aprelia 1906 goda s prilozheniiami* (Moscow, 1906), 2.

[84] For a description of the "mixed stratification system" in France, see Elinor G. Barber, *The Bourgeoisie in Eighteenth-Century France* (Princeton, N.J., 1955), 9–11.

[85] For provocative comments, see Rieber, *Merchants and Entrepreneurs in Imperial Russia*, 416.

[86] For the new description of estates, see Haimson, "Conclusion," 276–78.

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The survival of *soslovnost'*, even if in an attenuated form, remained a significant impediment to the political reconstruction of autocracy, as the disappointing fate of an official so energetic as Stolypin testifies.[87] Indeed, *soslovnost'*—as a legal principle, as *mentalité*—formed a crucial barrier to the political modernization of the old regime, for it was utterly antithetical to the creation of a modern civil society, which is a *sine qua non* for a democratic order. Hence, *soslovnost'*, if not *sosloviia*, survived to the end of the *ancien régime* and, in some significant respects, even into the Soviet period.[88] At least nominally, the *soslovie* system did not disappear until November 1917, when it was formally abolished by the new Bolshevik government. Indeed, that was one of the Bolsheviks' first acts—along with the more famous decrees on peace, land, and bread.[89]

I HAVE ARGUED FOR A FUNDAMENTAL reconceptualization of prerevolutionary Russian social history. As the belated development of the term *soslovie* suggests, the estate system arose in Russia much later than hitherto assumed. More important, the whole process was far more dynamic than most historians have realized. Indeed, the *soslovie* system was still developing as the Great Reforms drew near. Moreover, the flux—and confusion—in the state's terminology should evoke more caution toward assumptions about its central role in "legislating" social development; important as the state's influence may have been, other factors—cultural, economic, social—were of equal if not greater import. And, most striking of all, the notion of a *soslovie* system should not be fecklessly projected onto the eighteenth century. Before the early nineteenth century, authorities still thought of social groups as composed of individual males (as servicemen or taxpayers). Only gradually did they develop the conceptual apparatus for large social aggregates, and only in the early nineteenth century did they come to have visions of an ordered hierarchy of social estates.

Notwithstanding some tendencies toward aggregation and consolidation (chiefly among the privileged categories), the social structure did not coalesce into four large estates. On the contrary, it remained exceedingly complex and variegated, comprised of many distinct and often hereditary social groups, each possessing its own special status and constituting a separate *soslovie*. Although many of the traditional groups were formed in medieval Muscovy, others arose to satisfy public or private demands, and some, like the medical professions, emerged from the process of social and institutional development. Similar to the occupational estates (*Berufsstände*) of central European states, these modern social strata adapted to the existing order by assuming the *soslovie* form of the traditional hereditary

[87] For opposition to Stolypin from the nobility, see Weissman, *Reform in Tsarist Russia*, 148–220; and Diakin, *Samoderzhavie, burzhuaziia i dvorianstvo*, 101–04.

[88] The acute concern for "social origin" in the early Soviet state, especially the disfranchisement of entire categories on the grounds of their legal origin (the *lishentsy*), represented a kind of inverted *soslovie* policy until formal abandonment of the exclusion in 1936. As has indeed been noted before, in this and other respects Soviet social policy evinced some significant continuities—in principle, if not in application—with the *ancien régime*.

[89] *Dekrety sovetskoi vlasti*, 1 (Moscow, 1958): 71–72.

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estates (*Geburtsstände*). And in this process of *soslovie* formation, the operating dynamics were not merely state will but also collaboration by particular groups. Indeed, as educational and social policies suggest, the state often acted to counter excessive hereditariness and to facilitate greater social mobility.

Finally, these *sosloviia* did not inexorably dissolve into classes in the postreform era, as traditionally posited in the estate-class paradigm. Although some new groups, like workers, did tend to develop "class" identities and others, like the professions, endured an undefined "supra-estate" status, much of society still continued to think in terms of the prereform *soslovie* system. The strong hereditary patterns, the persisting legal distinctions, the segregation of groups in administration and law, the deeply rooted cultural differences among various groups, and the conscious effort of the state to preserve the *soslovie* separation—all acted to maintain the old social structure even in the face of far-reaching social and economic change. Hence, the *soslovie* system exerted a profound impact on late imperial Russia, where social relations—and social antagonisms—derived not only from economic conflict but also from the force of juridical-cultural stratification along *soslovie* lines. The complex, polymorphic social structure—a "mixed system" of estates *and* classes—made all the more difficult the plans of either the *ancien régime* or its political adversaries to mobilize and control society as the revolutionary crisis unfolded in the early twentieth century.

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Modern Education in South India, 1784–1854: Its Roots and Its Role as a Vehicle of Integration under Company Raj

ROBERT ERIC FRYKENBERG

> The advocates of improvement do not seem to have perceived the great springs on which it depends; they propose to place no confidence in the natives, to give them no authority . . . but they are ardent in their zeal for enlightening them by the general diffusion of knowledge. No conceit more wild than this was ever engendered in the darkest ages.
>
> Sir Thomas Munro, December 31, 1824[1]

BY THE EARLY DECADES of the nineteenth century, the East India Company's power in India had already become firm. Even so, the more perceptive among India's British rulers remained acutely conscious of the fragility of their tenure. They were so few and so different in culture from those indispensable hosts of subordinate officials upon whose loyalty and obedience they depended. The multitudes—remote in their ancient villages, distinct, even from each other, in their castes and customs—served to remind those among the British sensitive to the exercise of authority that their power was truly ephemeral. They knew that, without warning, unforeseen, unpredictable, and unsettling incidents could erupt. The dangers to central authority and rulership were all too manifest.

Most dangerous were measures that, however inadvertently, brushed against

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I am no less indebted to the historiographic context within which I have worked. Limitations of space do not permit fuller citation of the many pertinent works on connected historical issues that have been written by colleagues. Nevertheless, readers who may want to explore such related scholarly work should consult studies by Arjun Appadurai, S. Arasaratnam, Christopher A. Bayly, Carol A. Breckenridge, Richard Devitt, Nicholas Dirks, Dennis Hudson, Susan Bayly Kaufmann, Susan Lewandowski, David Ludden, Chandra Y. Mudaliar, Susan Basu Neald, Pamela G. Price, Burton Stein, R. Suntharalingam, David Washbrook, and others who have been searching out indigenous activities in the foundation and growth of imperial institutions, in the origins of cultural and organizational activity, and in those processes of social change under the Raj that gave rise to "public sentiments" and the mobilization of national consciousness. Only some of these connections are indicated in the notes below.

[1] "On the Employment of Natives in the Public Service," in Alexander J. Arbuthnot, ed., *Major-General Sir Thomas Munro, Governor of Madras: Selections from His Minutes and Other Official Writings*, 3 vols. (London, 1881), 2: 321 (hereafter, Arbuthnot, *Selections*).

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entrenched interests, deep prejudices, and religious feelings.[2] A casual slip or a thoughtless gesture could do more damage in a moment than could be repaired in years, leaving a miasma of distrust that never wholly subsided. Sudden outbursts of violence or disclosures of silent subversion—in Vellore, Goomsur, Canara, and Malabar, or in Coimbatore, Cuddapah, Guntur, and Nellore—could shake the complacency of ignorant or arrogant rulers, in turn confirming their worst prejudices and fears. Fears fed upon what was unknown, upon the dark and mysterious. In each division (*taluk*) and district (*zillah*), peculiar and strange, even bizarre, phenomena appeared "barbarous" or "uncivilized" to uninformed Europeans. Reports of Mappilah "outrages," Khond "sacrifices" (of *meriahs*), covert networks of brigandage-and-ritual murder (by *thags* or *phansigars*), pilgrim plundering and village burning (by *dacoits* or by pindaris), and other forms of banditry, rebellion, ritual suicide, and witchcraft flowed into secretariats from every locality. Enough reports came in—of secret combinations and open insurrections—to unsettle many who sat in high places.

Of all such reports, however, none so concerned British overlords as those that brought the loyalty of Indian officials into question. No other matters so concentrated British attention; none had implications more grave. The security and stability of the state, the rulership (raj) of the Company itself, depended on the loyal efforts of hosts of Indian servants, both civil and military. Without the faithful services of so many, the entire structure of empire could never have come into existence or continued to function.[3] And there was the rub. Given the complex and segmented nature of the social order in each locality, the ultimate priority each elite community gave to its own interests and values, the absence of appeals to larger nationalistic or patriotic feelings (beyond those of family, caste, and village), and, in short, the want of a common morality or a common humanity beyond loyalty to one's own community of kin and kind—on what grounds could one build a system of basic authority on which to erect a common ethic of loyalty?[4] Beyond the uncertain and shifting relationships of personal bond, professional contract, and patron-client obligation—of "true salt" (*namak hallal*) and troth plighted—what other obligations could cement together the disparate elements of such a vast imperial structure?

[2] Minute by the President in Council (Munro), Fort St. George, November 15, 1822, par. 8, Madras, Tamil Nadu State Archives (hereafter, TNSA), Madras Public Consultations (hereafter, MPC), no. 26, November 8, 1836, 657: 5403–58. Also see "Interference of European Officials in the Conversion of Natives," in Arbuthnot, *Selections*, 2: 302.

[3] Munro wrote: "It is strange to observe how many men of respectable talents have seriously recommended the abolition of a Native and the substitution of a European agency to the greatest extent. . . . Such a plan would not only render the character of the people worse and worse, but our Government more and more inefficient. . . . We are arrogant enough to suppose that we can, *with our limited numbers*, do the work of a nation." Arbuthnot, *Selections*, 2: 319.

[4] Was this social segmentation (and ethnocentricity) less true of the most menial, deprived, and ritually polluted groups? Had they been "socialized into profound self-abnegation"? Evidence on this issue is difficult to establish either way, since oppressed peoples are often reluctant to speak out, but data exist to suggest that the self-perceptions of the "low" were not always low. Studies touching this issue are far too many to list in full here, but some samples are noteworthy. See Michael Moffatt, *An Untouchable Community in South India* (Princeton, N.J., 1979); Robert Hardgraves, *The Nadars of Tamilnad* (Berkeley and Los Angeles, 1969); S. Saraswathi, *Minorities in Madras State* (Delhi, 1974), 103–80; and A. T. Fishman, *Culture, Change, and the Underprivileged* (Madras, 1941).

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It was this fundamental inconsistency, this inner contradiction between social plurality and imperial necessity, that began to exact a toll during the early decades of the nineteenth century. More and more "instances of extensive embezzlements, repeated malversations, and fraudulent combinations" against common authority came to light in every district and branch of government.[5] With penetrating insight, Thomas Munro observed (from Bellary in 1806), "It is surely a degrading spectacle to contemplate a great and civilized people fallen under a foreign dominion, with the first among them not only excluded from all power, but reduced in salary, even in the highest offices which they can hold, nearly to the level of domestic servants."[6]

How could one justify a parsimony that denied a fair reward to such respectable men? How could "any body of public men similarly situated in any country on the face of the earth . . . [having] such strong temptations . . . and so little inducement to honesty" remain loyal? Small wonder that every district collector faced "a general combination down to the lowest village servants" making it almost impossible "for him to learn what was going on" or, once "corruption" was uncovered, to effect a remedy.[7] Not all the devices that imagination might conjure, nor any amount of surveillance or punishment, could eliminate what was essentially a much deeper malady, a malady of conflicting values as profound between the segments of social elites within India as between Europeans and Indians. A decade later, after a searching enquiry into malfeasance, the Board of Revenue in Madras summarized its conclusions:

> We are far from believing that the artful intrigues, corrupt compacts, daring embezzlements, hardy frauds, and shameless perjuries of the Native revenue officers which have of late so much disgraced this department . . . can be traced to any depravity inseparable from the character of the Hindoos. We think they are to be attributed to very different causes: to the want of almost any inducement to resist the temptations to which they are exposed, to their defective education and consequent laxity of morals, and . . . to their uncertainty. . . . We are still further from thinking that these abuses have increased under the British Government.[8]

The extent of the problem was hardly surprising, considering that there were so few European officers and so much discretion remained with Indian officers, that "little more than a bare subsistence" was combined with "constant expos[sure] to the strongest allurements," and that "malversation [was] always easy and detection extremely difficult . . . seldom extending to half of the abuse." But, more

[5] Board of Revenue (hereafter, BOR) to Governor-in-Council (Hugh Elliot), December 11, 1815, par. 1, Madras, TNSA, Madras Revenue Consultations (hereafter, MRC), April 22, 1816.

[6] "Allowances to Servants in Taluk and District Offices," April 10, 1806, par. 59, *Selections from the Old Records of Bellary District* (hereafter, *ORBD*), TNSA, n.d., no. 10. For a reprint, see K. N. V. Sastri, *The Munro System of British Statesmanship in India* (Mysore, 1939), app. B.1, p. 20.

[7] "Allowances to Servants in Taluk and District Offices," pars. 55–56, *ORBD*; and Sastri, *The Munro System*, 15–17. Munro found that, during his seven years in the Ceded Districts of Bellary and Cuddapah, there were only six out of a hundred of his local officials against whom peculation had not been proved. But he hastened to explain that local people found nothing disgraceful in such conduct.

[8] BOR to Government of Madras (hereafter, GOM), December 11, 1815, par. 7, TNSA, MPC, April 22, 1816.

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important, Indian officials saw "nothing disgraceful in the pursuit of illicit gains except want of adroitness or success"; indeed, "established perquisites of office" were "justified by necessity."[9]

The solution lay in providing Indian servants with the incentives given European servants a generation earlier. Under Cornwallis and Wellesley, the European civil service had been transformed. Gone were the old days of venality, of "nabobs" and "pagoda-tree-shaking." Patronage no longer wholly determined position. Merit and training mattered. High emolument put the European official beyond easy temptation. Formal instruction and indoctrination, begun at Haileybury and Addiscombe and increased at the colleges of each Presidency, with awards and prizes to mark distinction, provided a common education, élan, and elitist esprit de corps that galvanized the Indian Civil Service and that later gave it such prestige. If one "steel frame" could be constructed on merit, why not another? Or better, why not enlarge the existing frame so that it could incorporate the most elite among Indians, the Brahmans and other notables in each locality whose loyalties were so essential to the whole imperial system?[10]

This was the vision inspired by the pen of Munro: "The remedy for this evil is the same here as in other countries; it is to place [Indians] above the necessity of betraying their trust."[11] The Board of Revenue concurred, "We must treat the natives with courtesy, we must place confidence in them, we must render their official situations respectable, and raise them in some degree beyond temptation, by making their official allowances adequate to the support of their station in society."[12]

The way to achieve this vision was through education, which seemed to promise more than all other remedies (pay, pensions, dignities, and honors) combined. Education could integrate the diverse elements of the Indian social structure and forge many conflicting loyalties into more common bonds. Education could bring about what nothing else had yet produced: a class of civil servants whose values meshed with political visions of a greater India. Yet development of education in Madras proved to be anything but easy. In this, as in so many other matters, Madras was already beginning to live up to its later reputation as the benighted "Cinderella of the East."

THE ORIGINS OF MODERN EDUCATION in South India lay in Northern Europe. It was there that new techniques evolved for building radically new social institutions and,

[9] Munro, Bellary Report, April 10, 1806, par. 56, *ORBD*; and Sastri, *The Munro System*, 16.

[10] Arbuthnot, *Selections*, 2: 320. Munro wrote: "We have instances of corruption among Europeans, notwithstanding their liberal allowances; but were the numbers of Europeans to be considerably augmented . . . this corruption would . . . increase. . . . Government could not possibly exercise any efficient control over . . . so many European functionaries. . . . No nation ever existed in which corruption was not practiced . . . by subordinate officers of Government; Munro, Bellary Report, April 10, 1806, par. 56, *ORBD*. For one notorious case of later corruption, see The Queen v. Alexander Maclean, Madras Supreme Court, (Original) Session: 1848, TNSA, GN 30338: 19.

[11] Sastri, *The Munro System*, 16–20.

[12] BOR to GOM, December 11, 1815, par. 16, TNSA, MPC, April 22, 1816; and Arbuthnot, *Selections*, 2: 320–21.

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therewith, for releasing unpredictable social energies. Precedents for "mass," "national," "public," even "popular" education came out of apocalyptic dreams and millenarian visions. From the inspiration of August Hermann Francke (1663–1727), Philip Jacob Spener's disciple, came actual experiments. Emphasis was placed on personal enlightenment, spiritual vitality, and moral transformation by means of a proliferating network of small training groups (*collegia philobiblica*). German Pietism, from its outset, generated radical impulses. The movement, advanced by Francke from Halle University, spread to the royal houses of Denmark and Hanover and the Court of St. James and helped generate the Society for Promoting Christian Knowledge (SPCK), which in turn promoted educational work in India.[13]

Bartholomaeus Ziegenbalg and Heinrich Pluetschau, Francke's students, brought his ideals to Tranquebar in 1706.[14] They made no secret of their distress with the human condition or with "godless" and "inhuman" behavior in every culture and clime. Their vision, forcefully set before them by Francke, contemplated nothing less than the total transformation of mankind through education, which they saw as the means to create a "universal realm of godliness."[15] Schooling for the poor, a mingling of the highborn with the low, equal opportunity for each deserving student of whatever means, and a broad curriculum including practical sciences and skilled trades were goals of the Germanic schools established along the Tamil Coromandel. From Tranquebar, where a Tamil printing press eventually turned out a steady stream of basic materials, such schools gradually spread northward to Madras, inland to Tanjore and Trichinopoly, and southward to Palamcottah. By the 1780s schools run by Christian Friedrich Schwartz and his associates (John Z. Kiernander, Daniel Joseph Jaenicke, Christian Wilhelm Gericke, John Kasper Kohloff, Philipp Fabricius, among others) were attracting non-Christian students, especially Brahmans and Vellalars, who hoped that the new learning would better qualify them for positions within the rapidly expanding establishments of the Company.[16] The training they received, especially their command of English, became so valuable that the Company began to give these schools annual grants of two hundred fifty pagodas and offered a similar subsidy to any other school that could serve the same function.[17] The schools run by Schwartz had been patronized by the rajahs of Tanjore, Ramnad, and Shivaganga for some time. Schwartz became *Raja-Guru* ("Royal Preceptor"), the guardian of the young rajah, and virtual regent of Tanjore on the death of the prince's father. Nor did these schools disappear when Schwartz died in 1799. Thomas Fisher's memoirs, published in 1827, indicate that a sum of 4,620 rupees a year was still being paid to them.[18] Since the Tanjore dynasty was Maratha, it may not be

[13] K. S. Latourette, *A History of Christianity* (New York, 1953), 894–97, 1018–35.

[14] E. Arno Lehman, *It Began at Tranquebar: A History of the First Protestant Mission in India*, trans. M. J. Lutz (Madras, 1956).

[15] F. Ernest Stoeffler, *German Pietism during the Eighteenth Century* (Leiden, 1973), 281.

[16] Hugh Pearson, *Memoirs of the Life and Correspondence of the Reverend Christian Frederick Swartz*, 2 vols. (3d edn., London, 1839), 1: 379–85, 2: 68–72.

[17] *Ibid.*, 2: 68–72: extract of despatch from the Court of Directors, February 16, 1787.

[18] "Fisher's Memoir" in A. N. Basu, ed., *Indian Education in Parliamentary Papers* (Bombay, 1952), pt. 1 (1832): 1–61; and Great Britain, *Parlimentary Papers* (hereafter, *PP*), 1831–32, IX (735–1), p. 412.

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farfetched to presume that some of the (Maratha Brahman) Desasthas who so quickly came to dominate the upper echelons of the Company's raj in the south were taught at these schools.

Meanwhile, a parallel development originated within the Male Orphan Asylum of Madras. From 1789 to 1796 this institution was under the care of Andrew Bell, a "reverend doctor" from Scotland. One morning Bell happened to be riding past a village school when he saw younger children being instructed to write in the dust by older and more advanced children. Much impressed, Bell organized a similar system of instruction at the asylum. This, not incidentally, freed him to obtain eight other sinecures, and he returned to Europe with "a pretty fortune . . . of no less than 25,935" pagodas. The remainder of his life he devoted to promulgating his "Madras System of Education." But Madras itself received few benefits from Bell.[19]

Except for the "Native Law College," about which we know little, the next attempt to set up modern schools came from Alexander Ross, district collector of Cuddapah in 1814 and a protégé of Munro. Alarmed at the "extensive embezzlements and fraudulent combinations" and aware of pleas for a "means best calculated to excite Natives in a faithful discharge of their public duties, by hope of reward rather than by dread of punishment," he reported what he knew of the traditional system:

> When the education which a young Native receives . . . comes to be considered, it can hardly be wondered at that he should turn out so ill. He is at an early age taught to read in one or two or three languages and to keep accounts. This is accomplished by the time he is thirteen or fourteen years old, after which he is considered as a burden on his family, and is turned out to advance himself in the best way he can. His grand object is to get himself a patron and a situation; and when he has accomplished the latter, he only tries to make the best of it; and from want of proper principles, he is not delicate in his choice of the means of doing so.[20]

Ross argued that the government should "take upon itself the education of its Native servants" and submitted a plan "for the proper education and moral instruction of young men, particularly though by no means exclusively Brahmins [ages twelve to twenty]." A school in each district could turn out fifty to sixty individuals each year who were trained in "those branches of knowledge required for the public service." Such training would "do more good to the Native character in general and go farther towards securing a faithful set of public servants than any code of laws or penal regulations on the face of the earth."[21]

Responses to Ross's plan, however, were far from wholehearted. Although he was praised by government officials, his proposal was shunted to the board of the College of Fort St. George. The Governor-in-Council, while allowing Ross to try his experiment in Cuddapah, curtailed his spending and cautioned against undue "confidence of success which, in the event of failure, might be found inconve-

[19] Charles Lawson, *Memories of Madras* (London, 1905), 205–10; and Bell, *Letters to . . . Sinclair, Bart., On the Infant School at Edinburgh* (London, 1829).

[20] BOR to GOM, December 11, 1815, par. 9, TNSA, MRC, April 22, 1816; and *PP*, 1831–32, IX (735–1), p. 412.

[21] BOR to GOM, December 11, 1815, par. 11, TNSA, MRC, April 22, 1816.

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nient."[22] Ross died before his program was established, but the Court of Directors in London had taken notice of its merit. In their despatch of May 22, 1818, they declared that "the establishment of seminaries for the education of the Natives, particularly Brahmins, as public servants, with a view to improving their moral principles and attaching them to Government, [is] a measure . . . well worthy of trial on a limited scale."[23] The court hoped Ross's experiment would continue, but its wishes went unheeded. Ross's death ended public service training in Cuddapah.

Meanwhile, at Palamcottah and Tinnevelly, James Hough, Company chaplain, opened an English school and fifteen Tamil schools. These reached a combined enrollment of 610 pupils. From 1816 to 1819, Hough used funds from his own pocket and obtained help from the SPCK and the Church Missionary Society (CMS) to erect two small schoolhouses, acquire books, and hire teachers. Classes in English grammar, composition, and arithmetic drew 59 students, mostly sons of Indian officers, civil and military. Hough maintained that the use of "Christian Scriptures" as teaching materials "excited no alarm in the minds of Natives." But when he asked for a monthly grant of twenty-five pagodas, because his schools were "a public benefit," he was turned down. The Government of Madras was not interested in schools of such a "private character."[24] Hough's efforts seemed doomed.

The arrival of C. T. E. Rhenius, however, completely altered the situation. Brilliant, controversial, and unconventional, he immediately set about increasing the number of Tamil schools. Calling for donations from both individuals and missionary societies, he did not wait for the Company to provide subsidies. Not hesitating to recruit gifted teachers, whether Brahman, Vellalar, or Christian, nor to enroll students without regard to sectarian barriers, he exploited the growing clamor for literacy and provided a new market for employment in education. In fifteen years, from 1820 to 1835, he established 107 small schools (with a combined enrollment of 2,882 pupils, 159 of whom were female), 2 secondary schools, and a training college for 40 scholars.[25] What happened in Tinnevelly District during this period, moreover, was only the most dramatic among several similar developments within the Madras Presidency. By 1858, the year in which Company rule ended, efforts by fourteen missionary societies had brought total enrollment at some 500 schools in South India to 38,607 students.[26]

It was in such a setting—of governmental indecision and missionary expansion—that Munro launched his comprehensive and far-sighted scheme. As governor of Madras (1820–27), he realized that literacy was confined to Brahmans, merchants, village lords, and "principle ryots." Women were almost totally

[22] *PP*, 1831–32, IX (735–1), p. 412; and TNSA, Nellore District Records, 3106: 129–35, October 5, 1816; Tinnevelly District Records, 3593: 117–19, May 21, 1816; Masulipatam District Records, 2963: 161–79, April 17, 1816.

[23] BOR to GOM, December 11, 1815, par. 93, TNSA, MRC, April 22, 1816. For the full text, see Sastri, *The Munro System*, 278–89.

[24] Secretary, Board of Superintendence for the College of Fort St. George, March 27, 1820, TNSA, Tinnevelly District Records, 3576: 39–56.

[25] J. Rhenius, ed., *The Memoir of C. T. E. Rhenius* (London, 1841), 357.

[26] *Proceedings of the South India Missionary Conference* (Madras, 1858), 11–19.

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excluded from elementary instruction. Munro had no wish to interfere with existing institutions: "People should be left to manage their schools in their own way." But, appalled by the prevalence of contradictory and ignorant opinions, which he saw as "mere conjectures . . . unsupported by any authentic documents about the ignorance of the people," he ordered a general survey. What was needed was a complete list of schools in each district, showing the number of scholars and teachers, castes to which they belonged, years of schooling they had received, books and materials that they used, kinds of specialized "colleges" (for astronomy, law, medicine, and so forth), and sources of funding (for example, whether by religious endowment or tuition).[27]

The results of this survey, together with complete census data, provide a detailed and fascinating view of education in 1823–25. There were 12,498 schools and 188,000 students in a population of 12,850,941—roughly 1 school per 1000 persons and 1 student per 67 persons. But, if we estimate that only half of the population were males and, of these, only a ninth (713,000) were five to ten years old, this means that there was 1 student for every 4 males of school age. Moreover, considering the uneven distribution of education between class strata (*varnas*), between urban and rural families, and between those taught or not taught at home, it is more likely that there was 1 student for every 3 males of school age. In Madras, for example, there were 26,983 males receiving instruction, with 5 males taught at home for every male not taught at home. Munro was not dismayed. "The state of education here, low as it is compared with that of our own country, is higher than it was in most European countries at a not very distant period."[28]

The reliability of reports and returns from this first survey—on which testing for validation is not possible—can all too easily be called into question. Nevertheless, the data, however rough and whatever the limitations, disclose some significant, even startling, insights. First, looking at relative geographic and regional distributions, one finds many more schools and students in Tamil than in Telugu districts. In each Telugu district, moreover, schools appear more Brahmanical and Sanskritic. Brahmans comprised 60 to 75 percent of all pupils, far outnumbering "clean" (*sat*) non-Brahmans—mercantile peoples (Balijas, Chettis, Komartis) and farmer-warrior lords of the villages (Velamas, Kammas, Reddis, Radzus).[29] "One invariably finds," wrote Abbé Dubois from his experience in Bellary, "that village schoolmasters . . . are Brahmins."[30] In Tamil country, on

[27] Minute on the Education of the Natives of India, June 25, 1822, TNSA, MRC, July 2, 1822; Arbuthnot, *Selections*, 2: 328–29; and Basu, *Indian Education*, 176–77.

[28] Minute on Education, March 10, 1825, in Arbuthnot, *Selections*, 2: 331–35; and Alexander Arbuthnot, *Papers Relating to Instruction in the Madras Presidency* (Madras, 1855), app. C: "Educational Census of 1825." District reports have yet to be subjected to a searching analysis. For comparisons with Bengal (1834–38), see William Adam, *Reports on the State of Education in Bengal*, ed. A. N. Basu (reprint edn., Calcutta, 1941). Adam claimed that 13 percent of "clean-caste" school-age boys were students in some one hundred thousand schools in a sample of 150,750 villages.

[29] Richard B. Devitt,"'For the Millions Who Surround Us': Company Education Policy in Madras, 1820–1848" (M.A. thesis, University of Wisconsin, 1977), and "Progress Report: South Indian Educational Census Data–Quantitative History Project" (typescript, University of Wisconsin, 1977).

[30] Abbé J. A. Dubois, *Hindu Manners, Customs, and Ceremonies*, trans. H. K. Beauchamp (3d edn., Oxford, 1906), 292.

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the other hand, Vellalar teachers predominated. Data would seem to suggest that, however high Brahman status was, many Tamil schools not only preserved the literary and inscriptional remains of an older non-Brahman (if not pre-Brahman or non-Sanskritic) heritage but also stimulated new creativity.[31] The Telugu heritage, meanwhile, remained largely excluded from the academy and confined to the oral epics of hereditary bards.[32] Thus, strong as Brahmans were in the extreme south, they enjoyed nothing like the monopoly over literacy and learning held by Brahmans further north.

Second, one notices the virtual exclusion of "unclean" or "untouchable" communities from education. The category "pariah," invariably found in other census reports, is conspicuously absent here. Munro, while noting that "mixed and impure castes seldom learn to read," claimed to have put a column into the survey for those among them who might. In fact, the survey only included columns for "Mussulman Scholars" and "All Other Castes." More significantly, the number of Brahmanical (Aryan or Sanskritic) social categories, the Kshatriya being a notable omission, shows the pervasiveness of Brahman influence. It was, after all, mainly Desasthas with whom Munro worked. Brahmans, after all, had made up the forms and then done much of the actual survey work. Moreover, survey figures themselves reveal that Brahmans and Vaisyas (the latter a tiny fraction) made up an average of 34.5 percent of all pupils in village schools. When one considers the hereditary nature of occupations, the "sacred-secret" nature of rituals and schooling in homes of the highborn in contrast to more open and casual forms of village schooling, and the customary obligation of payments to teachers, it is not difficult to imagine how strong the pressures of selective exclusion were. Whether in token fees or propitiatory offerings, in cash or in kind, a fee of three annas a month was far too much for most families, even those of "clean" pedigree "for whom the barest necessities require[d] the assistance of their children as soon as their tender limbs [were] capable of the smallest labour." In other words, the "unclean," whether Paraiyar, Pallar, Chakriyar, Mala, Madiga, or any other of the servile communities who made up half of the population, never counted.[33] Indeed, exclusive distinctions, reflecting the rituals of social stratification, pervaded all schooling. Even "caste" (or "clean") boys were separated, non-Brahmans and Brahmans sitting apart or in different rooms. Subjects studied, materials handled, and books assigned reflected this same segmentation. All pupils might be expected to memorize parts of the great epics—whether *Ramayana*, *Mahabharata*, or

[31] George L. Hart III, "Ancient Tamil Literature: Its Scholarly Past and Future," in Burton Stein, ed., *Essays on South India* (Honolulu, 1975), 41–63. Hints of a non-Brahman heritage in Tamil schools are found in works by Roberto de Nobili, C. Joseph Beschi, and early Tranquebar scholars. For a fuller analysis, see Dennis Hudson, "The Life and Times of H. A. Krishna Pillai (1827–1900). . . ." (Ph.D. dissertation, Claremont Graduate School, 1970), 18, 24–41. Also see John J. Paul and Robert E. Frykenberg, "A Research Note on the Discovery of Writings by Savariraya Pillai, A Tamil Diarist of Mid-Nineteenth-Century Tinnevelly," *Journal of Asian Studies*, 44, no. 3 (1985): 521–28.

[32] Gene H. Roghair, *The Epic of Palnadu: A Study and Translation of Palnāti Virulā Katha, A Telugu Oral Tradition from Andhra Pradesh, India* (Oxford, 1982). For many insights, I am deeply indebted to V. Narayana Rao, my colleague and Roghair's mentor.

[33] Arbuthnot, *Papers Relating to Instruction*, 45; and A. D. Campbell to BOR, August 17, 1825, par. 18, TNSA, MRC, August 25, 1823.

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Bhagrata. But status, creed, and function determined degrees of exposure and mastery of different kinds of lore. Those from "manufacturing castes," reported A. D. Campbell from Bellary, studied books "peculiar to their own religious tenets," and "those who [wore] the lingam" studied texts that "all considered sacred."[34] Simple forms of "agricultural accounting" or daily recitations in unison of memorized basic knowledge—"the sixty names of the years, the days of the week, the planets, the stars, the months, the important festivals and remarkable days"—could be performed by all. Indeed, from the *Amarakosha*, the renowned Sanskrit dictionary of synonyms, students also learned "names of the deities, the quarters, the . . . musical instruments, the divisions of the earth, the towns, the plants, animals."[35]

Education in the city of Madras could not have progressed as much as it did without the Madras School Book Society. This remarkable voluntary organization, formed in 1820 with active encouragement from the new governor, Sir Thomas Munro, consisted of prominent notables and officials, European and Indian. Its secretary was Vennelacunty Soob Row, a Desastha Brahman who had long served the Company and who ended his career as the head Marathi translator of the Sadr Adalat (later, High Court). Row put on record a clear, intimate, and thorough survey. As an insider, he understood the educational system and its prospects. After narrating his own experiences and providing details of how sons of elite families were trained for careers in the civil service, he sketched the rapid growth of local demands for advanced scientific and technical education. The desire for instruction in English, according to Row, had become so great that almost anyone with pretensions, however fraudulent, could set up a "school." He estimated that some five hundred "school" signboards could be found along Mount Road alone, each claiming to offer the "best" training in English. Indeed, the glut of such institutions had driven down the fees once charged for tutorial instruction. Yet, for all their pretensions, few of these so-called English academies could hope to compete with the hereditary systems of "sacred-and-secret" lore, which, when combined with personal patronage and family influence, enabled well-situated Brahman youths to find employment in offices of the Company's government.[36]

Row's own career serves as a classic illustration. He was born in Ongole in 1784 of a Maratha (Desastha) Brahman family of moderate means that had long held village titles to lands (as *mirasidars*) and occupied government positions. His schooling in Telugu began at age five. At ten, after his father's death and after his sacred thread (*upanayana*) ceremony, he accompanied a cousin to Masulipatam. There another cousin working "as a Gomashta [accountant] in the Sarishtah [Secretariat] Department" of the "Huzur Cutcherry [main office]" taught him how to copy accounts and letters. An uncle in Vinukonda who was "Manager of the

[34] Campbell, "On the State of Education of the Natives of South India," *Madras Journal of Literature and Science* (hereafter, *MJLS*), October 1834, pp. 350–59.

[35] V. Venkata Gopal Row, ed., *The Life of Vennelacunty Soob Row (Native of Ongole), Translator of the Late Sudr Court, Madras, from 1815 to 1829 (As Written by Himself)* (Madras, 1873), 67–68 (hereafter, *Life of Vennelacunty Soob Row*), 64–73.

[36] *Life of Vennelecunty Soob Row*, 64–73; and *Letters from Madras, By a Lady* (London, 1844), 16–25, 56–60, 92, 129.

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Postmaster's Office" tutored him further. Another stint in the Masulipatam Collectorate led to intensive study of English "under Bydepauty Venkatachellu, a Bramin." This in turn qualified him for work as a volunteer, copying English documents "under the benevolent protection of Navaloor Pareatomby Pillah, the Accountant in the Military Paymaster's Office" at Vinukonda. By 1799 Row was ready for his first proper position. At "one Madras pagoda per mensem" he served Henry Wilson at Guntur. As his English improved and more work came to him, he gradually became an employee of "Kompanee Bahadur." Sixteen years of achievement in many districts brought him to the Sadr Adalat with a monthly pay of ninety-five pagodas. By then, the list of his patrons, both European and Indian, had grown long. Row retired shortly after Munro's untimely death in Cuddapah, devoting his last years to providing patronage for young relatives and for religious endowments (for example, *pujas*, *shrotriams*, and *topes*).

The color and detail in Row's report can hardly be found elsewhere. From him we learn that promising apprenticeships were not possible without proper connections and proper skills in English. From him we learn that wealthier families employed English tutors while poorer relations went to special schools, either in Madras or in country towns. From him we learn that some families even left legacies for educational endowments and for passing on their sacred-secret lore, including private hoards of English language materials. From him we learn that such hoards included spellers, vocabularies, dialogues, readers, and literature (for example, *The Arabian Nights*). Row himself actively borrowed and lent books and liked to discuss Shakespeare and Milton with European friends. Curiously, however, formal study of English grammar was generally unavailable. Finally, it is from Row that we know that a boy could begin to learn English only if he could "obtain a small situation by the interest of his relations."[37]

Munro's sweeping scheme of 1826 clearly reflects ideas found in reports by such authorities as Row and Campbell. The shift in Munro's thinking in 1822, from passive noninterference to the most comprehensive plan for government involvement in education ever proposed (the like of which was not seen again for another century), is difficult to explain in any other way. What he advocated was nothing less than the building of a state-supported educational system, from remote villages to central city. "Better instructed teachers" were to be the pivot on which all else turned: "What is wanted is a school for educating teachers, as proposed by the . . . Madras School Book Society in their letter of the 25th October 1824." Next, each district collectorate needed two main schools, one for Hindus and one for Muslims, and, thereafter, (Taluk) Tahsildari schools as proper teachers became available. "This would, according to the plan proposed, give about forty Collectorate and three hundred Tahsildari schools."[38]

Munro had no illusions. Growth would, of necessity, have to be slow. Money for this scheme would also come slowly, until it reached, Munro hoped, forty-eight

[37] *Life of Vennelacunty Soob Row.* For corroborative detail, see "Company reliance on hereditary service: Evidence of Alexander Duncan Campbell," *PP* (General Sessions, H. C.: 1831–32): 174; and Dubois, *Hindu Manners, Customs, and Ceremonies*, 288–95.

[38] Minute (Munro), March 10, 1826, Arbuthnot, *Selections*, 2: 333.

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thousand rupees a year. Years would elapse before results could become visible. Yet "whatever the expense [incurred by] the education of the people [would] be amply repaid by the improvement of the country."[39] Campbell had repeatedly written that expense could never be a valid objection and noted how well the "Madras System of Education" had been received in Britain:

> The economy with which children are taught to write in the Native schools, and the system by which the more advanced scholars are caused to teach the less advanced, and at the same time to confirm their own knowledge, is certainly admirable.
>
> Europeans, in this respect, may with advantage take a lesson from Hindu simplicity. The shade of the spreading banyan tree, to be found in every village, is in this climate the most wholesome and convenient, as it ever will be the most appropriate and beautiful scene for the village school, and the sand beneath it renders stationery altogether unnecessary.[40]

Reaction from London was cautious, praising Munro's survey but asking for details about the quality of instruction. Munro's new Committee of Public Instruction (CPI), under the board of the College of Fort St. George and in cooperation with the Madras School Book Society, began work in 1826. Declaring that "the general improvement of the education of the people in the territories subject to this presidency . . . [would] depend as much upon the coincidence of feeling on the part of the people as on the munificence of the Government itself," the committee suggested that every measure would have to be divested of "the odium of innovation [in order to convince] the people to go along with the Government in the undertaking." Training "a body of efficient teachers" at a "central college" in Madras received priority. The committee proposal stressed three kinds of learning: vernacular; classical, that is, Sanskrit and Persian, among other languages; and modern, that is, English and science. Two candidates for the teachers' college were to come from each district, and Brahmans were given preference. Initially each candidate received a monthly stipend of fifteen rupees.[41]

JUST AT THIS CRITICAL JUNCTURE, Munro died. His death, July 6, 1827, on the very eve of his retirement when his presence in London might have been so crucial, had serious consequences. Official actions during the following decade make a sorry tale, a saga of feuds, cross-purposes, and paralysis. Neither of Munro's successors, S. R. Lushington and Sir Frederick Adam, had his stature. Both lacked creative imagination and the ability to grip larger issues. Hesitancy, indecision, and parochial interest marked their proceedings. Nor could they cope with the

[39] *Ibid.*, 335.

[40] The quotation is from Campbell's report of August 17, 1823. See Campbell, "On the State of Education," 354. Also see Campbell, "IV. On Native Education," *MJLS*, April 1836, pp. 110–11. In 1836 Campbell was the senior member of the board of the College of Fort St. George.

[41] Court of Directors (hereafter, COD) to GOM, May 18, 1825, par. 18, TNSA, MPC; Minute (Munro), March 10, 1825, pars. 7–9, TNSA, MPC; Arbuthnot, *Selections*, 2: 331–35; and H. Harkness (CPI secretary) to Collectors, June 24, 1826, pars. 3–7, TNSA, MPC. Also see Basu, *Indian Education in Parliamentary Papers*, 112–16.

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countervailing forces—Company directors, Munro disciples, Hindu notables, and Christian partisans—that impinged on educational policy.

With the first two groups we are already familiar. The Court of Directors in London and the Civil Service in Madras, each with its own conventions and perspectives, gradually kept changing to meet new conditions. Between 1828 and 1830 letters from the directors mingled caution with support for the Munro policy. The "imperfect state of Native education," defects in existing institutions, inadequate data about quality of instruction, local opinion, and shortages of good teachers and proper supervision were constant concerns. Praise for the Madras School Book Society was coupled with alarm over possible conflicts with the Committee on Public Instruction:

> [Efforts] to elevate the moral and intellectual condition of any people . . . concern the education of the higher classes, of persons possessing leisure and natural influence over the minds of their countrymen. By raising the standard of instruction among these classes, you would eventually produce a much greater and more beneficial change in the ideas and feelings of the community than you can hope to produce by acting directly on the more numerous class. Only then can a body of Natives [become] qualified by their habits and acquirements to take a larger share and occupy higher situations in the civil administration."[42]

The Court of Directors could hardly have expressed its social bias more plainly.

Disciples of Munro became disenchanted. During Lushington's governorship, they saw cherished goals either neglected or abandoned. Obstructed or displaced, some retired, and others went on leave, taking their concerns to London. Parliamentary enquiries over renewal of the Company's charter (1831–32) allowed them opportunities to voice their opinions. Each managed, with adroit but strong words, to defend Munro's scheme. Campbell read his entire 1823 Bellary Report into the record. This, with its rich data on the "quality" of instruction in village schools, had already brought him recognition.[43] John Sullivan, district collector at Coimbatore since 1816, bluntly chided authorities for failing to use their gifted Indian servants more fully. No educational scheme would work so long as "exclusion from all offices of trust and emolument" continued. Worse, Europeans still took credit for work actually done by their Indian subordinates, kept them in precarious tenure, and treated them without consideration. Worst of all, Europeans "never [thought] it worth while to consult them upon any of those measures . . . which [had] the interests of the Natives for their professed object. . . . Education of Natives on a sufficiently extensive scale [had] never been properly tackled." The Munro plan, "if followed up, would have, to a certain extent, furnished the means of giving a common education to the Natives of India." Indeed, Sullivan testified that, during all his years in India, "a very marked partiality to the English language [and] a great wish to learn" had been manifest

[42] COD to GOM (Public Department), September 29, 1830, pars. 29, 6, and "Fisher's Memoir," in *PP* (H. C., Sessional Papers, app. I, 1831–32), IX (735–1), pp. 461–64.

[43] COD to GOM (Public), April 16, 1828, par. 6, in Basu, *Indian Education in Parliamentary Papers*, 190–93; and *PP* (Appendix to Report of Select Committee, H. C., General Sessions Papers, 1831–32), IX (735–1), pars. 501–05.

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among his Indian officers. "The spread of English," Sullivan concluded, "ought to be attempted by every means; the Natives are fond of it, and acquire it with more facility than we do their language."[44]

Henry Harkness, after years as secretary at the College of Fort St. George and secretary of the CPI, stressed the high qualities of the Indians. "I do not know in what particulars they differ from Europeans. . . . I do not think them in any way deficient in intellect; and the better classes of them are a moral people. There is an erroneous opinion prevailing about Hindoo character; I think they are considered less moral than they really are."[45] Disillusioned by the lack of action, Harkness remarked on the futility of government schemes. The only progress he had seen had come from "instruction given by the missionaries, whose principal occupation [was] affording education to the children of the country."[46] Indeed, thousands of children attended these private institutions, while government efforts remained at a standstill.

Thus, early attempts to move the wheels of government failed. Munro's men, in the face of resistance at Fort St. George, lost momentum and became increasingly ineffective. Moreover, candidates for teacher training had not been carefully selected. None were properly qualified, either by age, skill, or inclination, and early results were disastrous. Only fourteen had been "certified," despite their failure, along with the remaining twenty-four applicants, to meet the required standards. Only fourteen of the forty proposed collectorate schools and sixty-seven of the proposed three hundred Tahsildari schools had ever opened. And these were reported to be virtually worthless.[47] Meanwhile, the Madras CPI had itself come under attack. In 1830, London began to support the "downward filtration" policy of Calcutta; in 1834, it offered to send out British teachers; and, in 1835, the Calcutta CPI not only rejected the Munro plan but also demanded an end to the collectorate and Tahsildari schools. This Sir Frederick Adam did at a stroke. In place of the Madras CPI he set up a new Committee of Native Education—made up of a Member in Council, a Company chaplain, a Maratha Brahman, and three officers of the Indian Civil Service. The committee proposed a limit of four schools and a "normal class" for teacher training to be adjoined to the best private English school. Henceforth, "by fixed rule, in all Government schools, the English language and European science and literature alone [would] be taught." But these drastic proposals, coming in 1836 when the Gumsur Wars and a change of governors preoccupied Fort St. George, were stillborn.[48]

The other two forces impinging upon educational policy were "private." That

[44] Evidence of John Sullivan, Esq. (Richings Lodge, February 21, 1832), *PP* (Appendix to Report of Select Committee, H. C., General Sessions Papers, 1831–32), IX (735–1), pars. 491, 501; and Basu, *Indian Education in Parliamentary Papers*, 265–67, 272, 292.

[45] Extracts from Proceedings of Parliamentary Committee, par. 1443, pp. 257–58, in Basu, *Indian Education in Parliamentary Papers*, 289.

[46] *Extracts from Proceedings of Parliamentary Committee*, par. 1953, p. 291.

[47] Arbuthnot, *Papers Relating to Public Instruction*, 11–15.

[48] COD to GOM, September 29, 1830, TNSA, MPC, February 5, 1834, November 24, 1834, May 18, 1836; and "Selections from the Records of the Madras Government," Arbuthnot, *Papers Relating to Public Instruction*, app. K, pp. 1–1v, app. J, app. P, xcii–xcviii, 30–33.

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is to say, two important blocs of special interest each sought to bend decisions of government its own way. Of the two, the "Hindu bloc" was by far the more powerful. Predominantly official, professional, and commercial, and essentially conservative in outlook, these Hindus saw themselves as part of the imperial establishment and as beneficiaries of its patronage and power. To that end they performed corporate worship (*puja*) in their temples. To that end they paid for expensive rituals, propitiated deities, purchased endowments, and reaped shares of honor and glory for themselves and their relatives.[49] To that end also they formed a "loyal opposition" to policies that they disapproved. To that end they felt free to memorialize and petition. And to that end, after 1830, their prayers focused more and more on education.

It is important, at this point, to understand how much the Company's raj in the south had become, at least in some measure, "Hindu Raj." This argument, which I have made more fully elsewhere, rests on our increasing knowledge of the "inner logic" of the constitution and structure of the raj. In simplest terms, the Hindu influence depended more on local de facto and local de jure institutions than on ephemeral attitudes among officials within constantly changing governments. One action alone, taken by the Madras government in 1817, suffices to show how the Hindu character of the raj became established. Under Regulation VII passed that year, the Company made itself responsible for the physical properties and for the proper functioning of every temple (and regular place of worship) throughout all its domains in South India. By this single step, the Government of Madras became the leading participant in and upholder of corporate worship in each locality. By implication this meant that, henceforth, those hundreds of thousands of coolies who annually pulled huge temple cars in the towns and villages, not to mention those who fell beneath the giant wheels, did so under official sanction. Such legislation did much to integrate the various institutional fragments of South India. By such actions the Great Wheel (*Mahachhakra*) of the new system of imperium now comprised more segments than had ever before been brought together into one structure.[50]

Looked at from the perspective of "Hindu Raj," two names above all others should be engraved on the cornerstone of modern education in Madras—Pachaiyappa Mudaliar and George Norton. Pachaiyappa never knew, and would have been shocked had he realized, the extent of his influence. Born near Madras in 1754 of a poor family, this astute Vellalar became a master *dubash* (agent, broker, go-between, or "double-language man"). After serving many merchants and princes, he left a large bequest for "charitable and religious" endowments, which became the object of prolonged litigation. In 1826 Sir Herbert Compton,

[49] V. Raghavan, trans. and ed., "The Sarva-Deva-Vilasa," *The Adyar Library Bulletin*, 22 (1958): 45–116.

[50] Frykenberg, "The Silent Settlement in South India, 1793–1859: An Analysis of the Role of Inams in the Rise of the Indian Imperial Systems," in R. E. Frykenberg, ed., *Land Tenure and Peasant in South Asia* (Madison, Wis., 1977), 37–53, "The Socio-Political Morphology of Madras: An Historical Interpretation," in K. A. Ballhatchet and J. B. Harrison, eds., *Changing South Asia: City and Culture* (London, 1984), 21–41, and "Religion and Company Raj in South India," *Fides et Historia*, 17, no. 2 (1985): 6–37.

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advocate-general of Madras Presidency, uncovered flagrant misappropriations by the executors and obtained a court injunction. At this point Norton became advocate-general. He succeeded in recovering a chest of jewels and a Tanjore bond worth nearly eight hundred thousand rupees. Ultimately the Board of Revenue became the beneficiary, with a strong recommendation from Norton that the bequest be used for educational programs. The board deadlocked over whether it could be used for anything other than "religious and superstitious" purposes. The deadlock might have continued had not a new governor, Lord Elphinstone, arrived and, at Norton's urging, issued peremptory orders. In 1839 a special charter was drawn up, incorporating the Pachaiyappa Charities as a permanent and self-perpetuating trust to be used for Indian education. Before a large assembly of notables, Norton formally delivered this charter, duly inscribed on a ceremonial parchment, to a governing body consisting "wholly of Native Trustees."[51]

This event also marked the culmination of years of effort by the Hindu gentry. Ever since Norton's arrival, forward-looking Indians (Hindus) had gathered at his house for weekly discussions "on subjects of general interest . . . , raising the zeal and cooperation of the Native public mind for the advancement of Education." These discussions led to formation of the Hindu Literary Society in 1830, and Norton's public lectures on modern education and "political privileges" inspired the society to open its first modern English school in 1834. The clamor from "respectable families" for English education had reached such a pitch that school facilities quickly became inadequate to the demand. By 1839, just as Pachaiyappa Charities came into being, this clamor had turned into organized representation, which could not be denied.[52]

The Education Petition of November 1839, drafted by leaders of the Hindu Literary Society (among them, Gazulu Lakshmanarasu Chetty, C. Narayanaswamy Naidu, and C. Srinivasa Pillay), contains the first glimmer of "public" consciousness in Madras. The petition circulated in English, Tamil, and Telugu and ultimately included over seventy thousand signatures. The text is evocative of local sentiment:

> My Lord, we are *the people* of this country, inheriting this land for thousands of generations. From our industry its wealth is supplied. By our arms it is defended from foreign foes. By our loyal obedience to the established Government its peace and safety are maintained. If diffusion of Education be among the highest benefits and duties of a Government, we, the people, petition for our share. We ask advancement through those means which . . . will best enable us . . . to promote the general interests of our native land. We ask it only in proportion to our long proved attachment to the British Government and its enlightened institutions. . . . We descend from the oldest Native subjects of the British Power in India;

[51] George Norton, *Native Education in India: Comprising a Review of Its State and Progress within the Presidency of Madras* (Madras, 1848), 27–30; C. S. Srinivasachari, *History of the City of Madras* (Madras, 1939), 238; and Evidence of George Norton, Esq., June 6, 1853, *PP* (General Sessions, H. L., 1852–53), XXXII, p. 91.

[52] Norton, *Native Education*, 23; Evidence of George Norton, Esq., June 6, 1853, *PP* (General Sessions, H. L., 1852–53), XXXII, pp. 91–92; R. Suntharalingam, *Politics and Nationalist Awakening in South India, 1852–1891* (Tucson, Ariz., 1974), 37, 26–27, 35–40; and Arbuthnot, *Papers Relating to Public Instruction*, app. K, p. lv.

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but we are the last who have been considered in the political endowments devoted to this liberal object.[53]

The Hindu gentry of Madras expressed dismay that, despite oft-repeated "benevolent intentions of the Court of Directors," nothing had been done. High schools and colleges like those in the "sister Presidencies" were what they urgently needed. Nor were they persuaded that "those objects [could] never be imparted, except through . . . interference with their religion"—obliging them "to act as if they renounced the religious faith in which they [had] been brought up."[54]

Certain radical Christian groups opposed the Hindu lobby. We have already noted that pietistic congregations and schools in Tinnevelly had multiplied rapidly after the turn of the century. Other Christian communities—St. Thomas (Syrian), Armenian, and Roman Catholic—had been in India much longer. But after 1793, especially after 1813, radical changes in style and rapid acceleration in numbers of conversions altered the social atmosphere. Relationships both inside and outside various communities, whether Hindu or Christian, kept changing, and, where security and status were concerned, such changes were bound to be disturbing. Mass conversions within the lowest, "unclean" castes, combined with attacks on "heathenish" customs, especially on caste as such, alerted some Hindus to a new level of Christian aggressiveness.[55]

In 1830 another form of Christian-sponsored education arrived. Out of the Scottish Enlightenment, from St. Andrews and the lectures of Thomas Chalmer, came the Scots Calvinism of Alexander Duff. English was no longer taught merely as a second language. No longer did each student learn mainly within his own cultural idiom. Duff's brand of education was aimed directly at "the influential classes" and conducted exclusively in English. He believed converts from "respectable" families, when properly trained, were the best guarantee of "truly Christian" and self-perpetuating congregations in India. As he put it, "The mind, in grasping the import of *new terms*, is perpetually brought into contact with *new ideas*, and *new truths*, of which these terms are the symbols . . . , so that by the time the English language has been mastered, the student is *ten-fold* less the child of pantheism, idolatry, and superstition than before."[56]

Duff saw Sanskrit itself as evil. To him, each of its concepts was inseparably linked to some idea or sentiment that indoctrinated a person into "a stupendous system of error" and falsehood. Duff's efforts were confined to Calcutta, but his associate, John Anderson, carried the new strategy to Madras. Aiming directly at "the influential classes" of Hindus whose clamor for modern higher learning was becoming so loud, he opened his Scottish Free Church School in 1837. With a completely English, completely imported curriculum (teachers, books, laboratory equipment, and so forth), this school offered the following: grammar, composition,

[53] Norton, *Native Education*, 32.
[54] *Ibid.*, 31–34.
[55] Joseph Mullins, *Missions in the South of India* (London, 1854); and Julius Richter, *A History of Missions in India* (New York, 1908), 174–78.
[56] William Paton, *Alexander Duff, Pioneer of Missionary Education* (London, 1923), 66.

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and writing; logic, mathematics, and accounting; sciences and political economy. It also required moral philosophy, theology, biblical literature, and "Evidences and Doctrines of Christianity."[57]

The new school aroused both excitement and apprehension, simultaneously meeting felt needs and touching raw nerves. Yet incentives were so strong that despite a number of conversions, which caused protests and occasional mass withdrawals of Hindu students, classes soon filled up again. Applicants were always waiting to get in. But anxieties mounted with each bout of conversions, which, as much as anything, galvanized Hindu protest and provoked petitions to the government. The conversions prompted urban gentry to ask that government funds be used for higher learning in English rather than for the basic and vernacular (village-Tahsildari-collectorate) school system of Munro. Indeed, such conversions may well have been a factor in generating pressure for diversion of the Pachaiyappa bequest so that a college exclusively under Hindu control could be established. Thus, neither the Education Petition nor Pachaiyappa's Charities can be understood without reference to the alarm stirred up by conversions in Anderson's school.

By the 1840s the lines of conflict had been clearly drawn, with favor swinging back and forth during successive governorships. The European community, both official and unofficial, was divided. In one camp were old supporters of Munro, Elphinstone, and the "pro-Hindu" establishment. In another camp were supporters of Lushington and Adam and others who were "anti-idolatry" and pro-missionary. Although these lines may be exaggerated and perhaps too neat, they existed.

By 1842 the Hindu influence had grown stronger. The foundations of the Pachaiyappa Education Trust and Madras University had been laid. The first of several Pachaiyappa schools had opened. With an annual budget of twenty thousand rupees, trustees were spending more than had been spent by the government in years. Buildings for six hundred students had gone up in Black Town. C. B. Powell, formerly a professor at the College of Fort St. George, had devised a modern curriculum, in English. With sound teachers, some of whom were European, success was assured. Rising prestige had brought enrollment to five hundred. Indeed, with success had come plans to expand from elementary to high school and collegiate levels. Norton had been asked to serve as ceremonial "Patron."[58]

Norton also became president of the fledgling Madras University. Establishment of this institution, announced on December 12, 1839, only a fortnight after submission of the Education Petition, owed as much to Lord Elphinstone and

[57] Arthur Mayhew, *Christianity and the Government of India* (London, 1929), 134–35; Mohammad Mohar Ali, *The Bengali Reaction to Christian Missionary Activities, 1833–1857* (ChiHagong, 1965); and Bruce T. McCully, *English Education and the Origins of Indian Nationalism* (New York, 1940), 127.

[58] Evidence of George Norton, June 6, 1853, *PP*, (General Sessions, H. L., 1852–53), XXXII, pp. 91–92.

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Walter Elliot (the governor's cousin and private secretary) as to Norton. All three dreamed of bringing higher learning to the "respectable elements" of Madras society. In this they were joined by Powell and Peter Pope, secretary of the university's board. Published annual reports show to what lengths this group went and with what care they hoped to build the new institution. Guidelines, established on February 12, 1841, made plain that there would be no "frittering away" of funds on wasteful sinecures and country schemes, no settling for less than "a collegiate institution" even if it was cautiously begun as a "High School," and no exclusion of "Native cooperation" in planning or in governance.[59] Norton himself put enormous effort into the institution (as did Pope and others). He made sure that half of the University Board's fourteen members were Indian notables. He tried to make sure that future appointments to government service would go to university graduates, who would earn "Test Certificates" before being labeled "Proficients" of Madras University. Hindus on the board, despite this "not inconsiderable interference in their official patronage," acquiesced on being assured that university standards would be so high that only the highest castes would ever qualify. English and high tuition alone would effectively bar "the lower orders."[60] As Norton put it, "*Light must touch the mountain tops before it pierces to the depths.*"[61] To make sure that claims would not exceed actuality, Norton insisted, to begin with, that only a "preparatory school" be started. Finally, on April 14, 1841, "amidst a vast multitude of the assembled population, collected within and without the College Hall," Madras University's high school formally opened. This new institution immediately became the city's most prestigious school.[62]

Plans for expansion to full university status followed. These included construction of University (Presidency) College, an engineering college, a medical college, and four provincial (high) schools. The board called for "the mental improvement of the upper classes of the Native Community, who [have] the leisure and means to pursue the higher branches of study in European literature, science, and philosophy, as well as Native learning and languages." Moreover, to allay Hindu fears, they specifically prohibited religious teachings or anything "distasteful to the Natives generally, which might be fatal to the very objects sought." These plans came to the governor's desk for final approval in May 1842.[63]

Unfortunately for proponents of expansion, the Marquis of Tweeddale had just succeeded Lord Elphinstone, and the new governor held altogether different views. Old soldier of "the Duke," supporter of the Scottish Church, and manager of his ancient estates, he favored "practical knowledge." A long silence prompted an uneasy Elliot to write to Elphinstone:

[59] Minutes of Lord Elphinstone, December 12, 1839, February 12, 1841, London, India Office Library and Records (hereafter, IOLR), Elphinstone Collection (hereafter, Elphinstone Papers), Eur. MS. F/87.

[60] Norton, *Native Education*, 44–52.

[61] Arbuthnot, *Papers Relating to Public Instruction*, 47.

[62] "The Opening of the Madras University," *Annual Reports of Madras University* (Madras, 1842), app. 3; and Norton, *Native Education*, 47.

[63] *Annual Reports of Madras University*, app. 1, pp. 41–47, app. 3, pp. 49–52.

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Norton is in error if he supposes he made a favorable impression in his interview. The Marquess quotes his speech "On Education of 25 minutes duration" . . . when he has some other thing to do, with by no means a convinced air, whenever the subject is alluded to. When the reply of Government comes down, I mean to protest against the pause and delay . . . that the Medical College, the Engineering School and the Provincial Schools may go on.[64]

Tweeddale conveyed altogether different opinions to London three months later:

I have attended the Schools, and College Examinations. If India is to be educated by these means, neither you nor I will see much advancement in our day. If you were to nominate three Gentlemen who have been accustomed to the system of practical education in England and Scotland, you might have a chance of succeeding; but making the Natives rehearse Shakespeare . . . seems to me absurd.[65]

For students to explain the parts of a steam engine "like so many parrots" was disappointing. He felt "Private Schools" were more developed and, hence, "on a better footing." As for the university plans on his desk, Tweeddale declared, "I believe the Advocate General wrote the Minutes on Education supporting Madras College. He has its success very much at heart; but I think his views extremely visionary." After fifteen months, he forwarded the university's budget to London for approval. He told Leadenhall Street that the plans were "premature" and that expansion to include medical and engineering colleges would entail "excess of the amount sanctioned for educational expenditure at Madras."[66]

Tweeddale's tactic took its toll. Official delay and obstruction led to dismay, disgust, and resignations from the University Board. By 1845 no quorum remained. But, by mid-1843, London was also beginning to chide the governor. How could he present awards at Anderson's school and yet, in the same month, not even attend annual prize ceremonies at the university high school? Elliot, writing to Elphinstone, reflected on the gathering gloom:

On the subject of education, we are in a very unhealthy condition. Not a letter has been answered during the last ten months; and all our prospects for the provincial schools, the medical and engineering colleges &c are in abeyance. We did not even get leave to print our annual report and not a single vacancy among the governors, Native or European, has been filled up. Norton has become disgusted and . . . irritable on all matters connected with the cause. We have just lost Pope, a much more severe blow than the loss of Government patronage. Powell is a most invaluable and excellent man. He devotes his whole time and energies to . . . imparting the first rudiments of instruction . . . ; so that having rolled the stone a few steps up hill, the poor man finds he has again to commence from the foot of it.[67]

By November, when the text of his despatch was finally released in Madras, the gist of the governor's response evoked sarcastic mimicry from Elliot: "'I have

[64] Sir Walter Elliot to Lord Elphinstone, November 9, 1842, Elphinstone Papers, J. 10 (D-41).
[65] Tweeddale to Lord Fitzgerald and Vesci, February 23, 1843, IOLR, Tweeddale Collection (hereafter, Tweeddale Papers), Eur. MSS. F/96, Home Private Letter Book, pp. 84–87.
[66] Arbuthnot, *Papers Relating to Public Instruction*, 77.
[67] Elliot to Elphinstone, September 22, 1843, Elphinstone Papers, J. 10 (D-41).

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watched the progress of National Education in Europe for the last twenty years. . . .'"[68]

By this time, all patience was gone. An exasperated University Board publicly blamed Tweeddale for declining enrollments, resignations, and loss of support. Tweeddale refused the board's annual report, returned it in "corrected" form, and demanded signatures. When the board members then refused, he dispatched his own "corrected" version to London. The Court of Directors tried conciliation. There could be no objection to costs, since restrictive admissions and high tuition had brought a budget surplus of one and one-half million rupees. Nor could one object to the principle of education for Indians, since the scheme of "preference for proficients" in government service was working. Only the issue of expansion remained questionable. Doubts about the "stability" of the university and about whether "the High School department [was yet] fully organized and efficient" enough to enable work at the collegiate level prompted the court to concur with the governor's assessment that expansion was "premature."[69]

Such words brought "very painful mortification" to Madras people.[70] After years of carefully nurturing consensus among authorities in London, Calcutta, and Madras, Elliot made no effort to disguise his deep dismay:

> The Court's despatch on the University is such a damper that I feel sick of the whole thing and have resigned my place on the Board. Norton means to send a reply, and if he gets no reply, will follow my example. Besides the Natives have not been true to themselves. Streenevassa Pillay is a good for nothing fellow and an ass to boot. Ragavah Charriar and Cholapah Chetty, the two best men, are dead; and none of the Native Governors have exerted themselves. . . . If they would support the School and bring boys, we might carry on in spite of discouragement from Government, and by improvement command success. But we tried in vain to move them and now the school is dwindling into nonentity.[71]

Tweeddale next began stripping functions away from the University Board. A new body—the Council of Education—was set up early in 1845 and became responsible for elementary education in all of the remaining state-supported schools. Composed entirely of Europeans sympathetic to "Christian" causes, its very nature provoked trouble. Initially the council did nothing to aggravate the old board, and, indeed, the board welcomed the possibility of relief from burdensome supervision over out-station (*mufassal*) schools. But this uneasy cooperation did not last. In December the new council was made all-powerful, and the old board was ordered to "scrupulously abstain from adverting to [expansion]" and to limit itself strictly to supervision of the high school.[72]

At issue was the question of who would control the first public "uncovenanted civil service" examinations ever held for Indians in Madras. The Tweeddale government announced that these "Test Examinations," scheduled for March 1846, would be supervised by the Council of Education. But the tests were

[68] *Ibid.*, November 25, 1843.

[69] Arbuthnot, *Papers Relating to Public Instruction*, 78 n., 86–87; Norton to Select Committee, House of Lords, June 15, 1853, *PP* (1852–53), XXXII, pp. 101–11; and Norton, *Native Education*, 63, 68.

[70] Norton, *Native Education*, 63.

[71] Elliot to Elphinstone, January 23, 1844, Elphinstone Papers, J. 10 (D-41).

[72] Norton to Select Committee, House of Lords, *PP* (1852–53), XXXII, pp. 101–11.

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boycotted. This boycott cannot be understood without recognizing the acute anxieties experienced by some of the Hindu gentry in Madras. Rumors spread through clandestine information networks and amplified concern in Madras over government reactions to Hindu-Christian troubles in Tinnevelly. Through informers, office clerks, and family contacts also came word that certain questions in the examination "related to the Christian religion, its superiority, influence, &c [which would] give offense to Hindus."[73] Worse, "public" prizes would be offered in Greek and Latin, languages that could only be learned at one private grammar school, which further prejudiced the competitive positions of most Hindu students.[74] When accused of tampering with standards that Madras University had set, Tweeddale replied:

> The Governor in Council does not attach importance to a minute acquaintance with Shakespeare and other refinements of English literature, or to high scientific refinements, which appear to him equally beyond the requirements of the present day. The tendency of such a system appears to him to create acute and subtle, but with all, superficial, rather than solid, moral and thinking minds, which last are the only foundation of real social progress, and of a vigorous national intellect.[75]

As was to be expected, the examinations were held as scheduled. But only a handful of local people, mainly from lower castes, came forward. The event was a failure.

Meanwhile, Tweeddale filled vacancies on the University Board with ten members that he hand-picked. His aim, it seems, was to undermine the school. The new board drastically reduced tuition and then revived expansion plans—but not for engineering or medical schools. Instead, a grammar school was to be attached to the university. As hundreds of new students flocked to register, the governor expressed satisfaction that, at long last, "a sound practical system of education [was being] established and received by the Native community."[76] As far as he was concerned, years of development would be needed "before any benefit could be hoped for in the further establishment of college classes."[77]

Consternation now reached a fever pitch. Letters to newspapers, signed by hundreds, and petitions, signed by thousands, sounded new alarms. Norton, outraged that this "inundation of the lower orders [meant] nothing less than the overthrow of the institution," spoke plainly. A university "has totally distinct objects from the *elementary education of the masses*, which may be more strictly considered *national* education. It is entirely and solely directed to the superior education of the higher orders; and it would be ruinous to its existence to identify

[73] *Ibid.*, 67, 70–76, 111.

[74] *Proceedings at the Public Meeting of the Hindu Community, Held in the Rooms of Patcheapa's Institution, on Wednesday, 7th October 1846* (Madras, 1846), 7–8.

[75] Arbuthnot, *Papers Relating to Public Instruction*, 73.

[76] "Minute of the Marquess of Tweeddale on Education and the Introduction of the Bible as a Class-Book, With a Copy of a Despatch from the Court of Directors Related to the Same," Appendix to Sixth Report of the Select Committee . . . on the Affairs of East India Company, 1853, *PP* (General Sessions, H. C., 1852–53), XXIX (897), pp. 189–92 (hereafter, App. Sixth Report, H. C., 1853).

[77] Arbuthnot, *Papers Relating to Public Instruction*, 76–79.

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or amalgamate its discipline and objects with those of elementary schools."[78] Indian members of the university board, "one and all," fully agreed with Norton. He warned that there would be "an appeal to the Native public in order that the feelings of the Native community might be manifested.[79]

At this point, in August 1846, Tweeddale announced further plans—to introduce the Bible as a "textbook" in all "national" schools. He explained, "It is the only means I know of giving to the Natives a practical knowledge of the sources from whence arise all those high qualities which they admire so much in the character of those whom Providence has placed to rule over them."[80] His action drove even his staunchest supporters in Madras and London into opposition. Sober missionaries deprecated his "rash act" as dangerous and wrong. Conversion, after all, was internal and voluntary, not something imposed by fiat. John Tucker, secretary of the CMS in Madras, later recalled how this one act split the missionary community.[81]

A few weeks later a girl attending the Scottish Free Church School declared herself a Christian and sought asylum. The girl's family, claiming that she was but a child "captured" and detained "by force and fraud," got the court to serve a writ of habeas corpus. When Anderson and the girl came before the Supreme Court, many in the packed courtroom shouted in outrage—abetted by the tumult made by thousands outside. One overwrought relative even tried to seize the girl. The presiding judge-magistrate, Sir Edward Gambier, had to decide whether the facts of the case constituted a misdemeanor—"when a child *under years of discretion* [was] either taken away or detained against the consent of parties having charge of it."[82] Whichever way he decided, further appeals were inevitable. When two European doctors assigned to examine her (one of them the governor's personal physician) declared that she had indeed reached "years of decision," Gambier awarded custody to Anderson. For the Hindu community, however, the ultimate disgrace had already occurred. A high-caste girl had been defiled. This event, following similar recent conversions and defilements of Brahman schoolboys, brought already smoldering resentments to the point of combustion.[83]

More trouble, meanwhile, had broken out in Tinnevelly. On November 11, 1845, a large crowd had attacked Christian congregations. At Nullur, hundreds of houses had been destroyed, property taken, men beaten, and women violated. Organized by agents of the Vibuthi Sangam and Chatur Veda Siddantha Sabha

[78] Norton, *Native Education*, 59–60.
[79] Norton to Select Committee, House of Lords, June 15, 1853, *PP* (1852–53), XXXII, p. 110.
[80] Minute, August 24, 1846, App. Sixth Report, H. C., 1853, pp. 189–91.
[81] Evidence of Rev. John Tucker, July 28, 1853, *Ibid.*, 99.
[82] H. C. Montgomery, August 9, 1847, Proceedings Relative to the Native Girl Mooneatha, IOLR, Madras Judicial Consultations (hereafter, MJC); London, IOLR, Collections of the Board of Control or Board's Collection (hereafter, BC), 112.092; and George Norton to GOM, July 7, 1847, *ibid.* The Supreme Court of Madras was a court of the British Crown, not to be confused with the Sadr Adalat, which was the Company's court of highest appeal (and also a court of original jurisdiction) in South India. Appeals went from it to the Judicial Committee of the Privy Council in the House of Lords. Both courts were merged in 1861 to become the Madras High Court.
[83] Proceedings in Supreme Court on Two Writs of Habeas Corpus against the Rev. Mr. Anderson, BC, 109.461.

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in Madras, this violence brought the arrest of one hundred persons.[84] Those convicted in Tinnevelly had appealed, and their appeals came before the Sadr Adalat just when other grievances in Madras were reaching flash point. In May 1846, one judge—G. J. Waters, the only pro-missionary judge on the appellate bench—upheld the Tinnevelly convictions, but another—Malcolm Lewin, vehemently Unitarian, anti-missionary, and prone to involvement in sectarian disturbances[85]—joined a third judge in reversing Waters's ruling and throwing out the Tinnevelly convictions. At this point, the Government—consisting only of the Judicial Member who was the Chief Judge of the Sadr Adalat and the only Member of Council in town—ordered that all transcripts be turned over for review. This the judges refused to do, on grounds that such an intrusion by the executive into judicial proceedings was "unconstitutional." Tweeddale, far away in Ootacamund and caught in the middle, then took a step without precedent even in the checkered history of Fort St. George. Without consulting other Members of Council, he summarily fired all three Sadr judges. The uproar lasted for months.[86]

Within the Company's tangled origins as a business corporation and within its tortuous constitutional history, the power of a governor to dismiss any subordinate "for cause" had never before been questioned. But, since the Indian Law Commission itself had recommended that all Europeans be equally subject to Indian courts—"We are moving along the path toward . . . making our laws applicable to all classes of our subjects"[87]—many in Madras now claimed that even a governor could not violate such basic constitutional rights.

Mounting tensions in 1846, further aggravated by "leaks" of official documents to local newspapers, finally burst into the open.[88] The entire "public" of Madras took sides. Sympathy for abused Christians clashed with sympathy for abused Hindus. Each side righteously appealed to "free conscience" and basic "civil rights." On two issues—condemnation of the Government for its stand on the use of the Bible as a textbook in government schools and its dismissal of the Sadr judges—almost all agreed, with only *The Native Herald* of the Scottish Free Church Mission in stubborn isolation.[89]

[84] Robert Eric Frykenberg, "The Impact of Conversion and Social Reform upon Society in South India . . . , With Special Reference to Tinnevelly," in C. H. Philips and M. D. Wainwright, ed., *Indian Society and the Beginnings of Modernization* (London, 1976), 207–10; and "Outrages in Tinnevelly Occasioned by Bad Feeling Between the Hindoos and Native Christians," MJC, no. 2, February 10, 1846, in BC, 101.675.

[85] E. F. Elliott to J. F. Thomas, GOM, MJC, no. 54, August 11, 1847, BC, 109.460. Lewin's trouble began during Elphinstone's tenure. On Lewin and Waters, see Elphinstone Papers, Bx 2–C, 3–D.

[86] Memorial of Lewin and Boileau to COD, MJC, 505: 2849; Minute (Lewin), April 18, 1846, MJC, June 9, 1846, 506: 3231–59; and "Papers Relating to the Removal of Mr. Lewin from the Office of Judge of the Sudder Dewanny Adalat . . . ," *PP* (General Sessions Papers, H. C., 1852–53), XXVII, (426), pp. 551–52.

[87] "Minute by the Hon. Sir T. H. Maddock, Knt. . . . ," August 24, 1845, *PP* (Appendix to Report of Select Committee, House of Lords, 1852), 357; and *The Madras Spectator*, September 24, 1846.

[88] "Abstraction of Official Papers from Government Offices, Whole Secret Letter of 1st March 1848, &c," MJC, BC, 113.420.

[89] For letters from Hindus at Tinnevelly, see "Petitions . . . [re] Disturbances in Tinnevelly," in GOM to COD, May 8, 1848, MJC, BC, 114.783; TNSA, MPC, no. 20, August 10, 1847, 814: 3579–3601; TNSA, MPC, no. 52, March 23, 1847, 818: 1169–71; *Madras Native Herald*, June 5, 1847, in MJC, BC, 112.092; *The Madras Spectator* (1846–47); *The Atlas*, September 21, 1846; *The Crescent* and *The United Services Gazette*, in *PP* (General Sessions Papers, H. C., 1852–53), XXVII (426), pp. 566–69.

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Pent-up exasperation boiled over. Leaders of the Hindu Literary Society—G. Lakshmanarusu Chetty, T. Narasinga Rao, and others—organized a mammoth "public meeting of the Hindu Community." Thousands assembled at Pachaiyappa's Hall in the evening of October 7, 1846. So huge was the crowd that the Madras sheriff used mounted police to assure order.[90] A committee of notables was elected and charged with drawing up a petition to submit directly to the Court of Directors in London. The committee members accused the East India Company of "ingratitude, partiality, and injustice" in its dealings with loyal Indians without whose help it could never have attained supremacy over India. They accused Tweeddale's government of aiding and abetting the work of missionaries and pointed to the deliberate erosion of those special positions and privileges of patronage that elite Hindus had always enjoyed under the Company's raj. The committee predicted, "This evil must spread, unless confidence is restored; and the proceedings which have lately taken place at the Presidency are of a nature not to check, but to accelerate, the progress of disaffection and turbulence."[91] Once completed, three drafts of the petition were then circulated for signatures. When over twelve thousand people had signed them—in Tamil, Telugu, and Roman scripts—the drafts were formally presented to the Government for transmission to London.

LIKE THE GIANT WHEELS OF JAGANNATH, the machinery of the Indian Empire moved slowly. Decision-making processes sometimes seemed to stand still. Each potential advance in education met deadlock. Some of the earliest enlightened policies had begun in the south, only to end in frustration. Whether "upward percolation" from village notables, as Munro had hoped, or "downward filtration" from city gentry, as Norton hoped, both policies would seem to have been crushed under the Great Wheel. Yet was this really so? The central argument of this study is that it was not. What happened was *itself* policy—a balance wheel of forces. Education served as an instrument for slowly drawing together elements of a fractured social system. That no side won, that no one group fully succeeded, stimulated a growing social stability and a continual cementing of newly educated (hence socialized) elements into a new metropolitan society. These newly educated—high and low, "clean" and "unclean," Brahman and non-Brahman, Hindu and non-Hindu (Muslim, Christian, and so forth)—each found some sort of niche in this new society. However slight the consciousness of a single pan-Indian society may be in terms of the whole population, and however fragile the ties that bind many groups together, both the awareness and the ties are far stronger now than

[90] Minute of Henry Chamier, July 26, 1847, par. 17; Report of E. F. Elliott, August 11, 1847, MJC, BC, 109.460.

[91] "Complaints of Natives on the Subject of Religion," MJC, BC, 109.457, pp. 90–92. This manuscript is the only original copy of the "Hindu Memorial." It has over one hundred eighty pages of signatures: seven thousand in Tamil, three thousand in Telugu, and two thousand in Roman (English) script. Caste names and scripts come in blocks (indicating residential quarters); each has a suffix "*chevralu*," or "by the hand of," and looks antique and unique enough to be genuine. All names are Vaishnava (or Sri-Vaishnava).

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they were two centuries ago. India did not then exist as a single society, nor does it today. Its pan-Indian cultural and syncretistic potential were certainly there. But India was then still in the process of being made. And common education, whatever its form, was essential to that making.

The result in Madras was a curious blend found nowhere else in India—the only presidency where private enrollments, especially at primary and secondary levels, far exceeded those of government schools.[92] Advocates of state support wanted more aid devoted to higher (English) education in order to preserve elite dominance. Patronage, privilege, and prerogatives were for the "clean"—city gentry and country cousins. "Higher orders," having so long ruled the land and being naturally conservative, adroitly sought to keep their hold on modern and higher education.[93] More radical advocates of "national" education wanted to combine basic literacy, practical skills, and "moral" (ideological) training to mobilize all levels of society. If both "unclean" and "clean" were educated, then hereditary privilege, as epitomized by caste, would weaken, if not end. Opportunity for self-improvement had to be for all, but especially for the "lower orders." Radical "outsiders," mainly missionaries and converts, wanted to change the rules by which the whole social system functioned.[94]

Yet, for all of the apparent polarization between Christian and Hindu, neither extreme was "representative." Neither could command events. The supposed "Hindu" community was not broad enough to speak for enough "Natives." Indeed, the Government attributed some "Hindu" stirrings to a tiny group of "interested" Europeans.[95] Moreover, analysis of names on the petition of 1846 shows that many signers were not only non-Brahman but apparently Vaishnava.[96] Their support of Lewin, the Unitarian, is as understandable as their lionizing of Norton. Norton, far from being anti-Christian, believed more conversions in India, albeit by "fair" methods, would be beneficial.[97] At the same time, the so-called missionary interest was not monolithic. Many Europeans in Madras were alienated by what they saw as the "uncivil" behavior of some in their midst.[98] Aggressive, discourteous, dishonest, unfair, or even anti-caste actions were seen as "un-Christian" by upper-class Indian Christians of Madras and Tanjore and by schismatic "Hindu-Christian" congregations in Tinnevelly (alienated by Anglican treatment of Rhenius).[99] Concurrently, some "Hindu" groups, miffed at outside tampering with

[92] *Memorial of the Church Missionary Society. . . .* (London, 1853). For comparative enrollments, see *ibid.*, pp. 25–39.

[93] "Hindu Memorial" (Petition) to COD, May 12, 1846, par. 26, MJC, BC, 109.458.

[94] "Memorial of Christian Inhabitants of the Presidency of Madras [to COD], September 21, 1850, *Proceedings of the South India Missionary Conference* (Madras, 1858); *Memorial of the Church Missionary Society*, Note V, pp. 57–65; and Elijah Hoole, *Madras, Mysore, and the South of India . . . : 1820–28* (2d edn., London, 1844), 108–09.

[95] E. F. Elliott to J. F. Thomas, MJC, no. 54, August 11, 1847, BC, 109.460; and COD to GOM, MJC, no. 4, December 1, 1847, BC, 113.374.

[96] For the original petition, see MJC, BC, 109.457. I am indebted to my colleague V. Narayana Rao and to the late B. Gopal Reddy and others at Tirupati for such insights.

[97] According to Norton, conversion would bring the "inevitable extension of the pure morality and holy spirit." See Norton, *Proselytism in India: The Questions at Issue Examined* (London, 1859), 20, 38, 44.

[98] Evidence of F. J. Halliday, July 25, 1853, *PP* (H. C., 1852–53), XXIX (897), p. 56.

[99] Petitions, March 7, April 18, June 24, July 15, 1834, February 27, 1835, TNSA, MPC, vols. 619, 620, 623, 633; and William Taylor, *Memoir of the First Century of the Earliest Protestant Missions at Madras* (Madras, 1847).

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promising "Christian" institutions through which some had gained employment for themselves and schooling for their children, condemned the Tinnevelly "outrages" and defended government and missionary actions.[100]

Thus, significantly, no interest was exclusively "Hindu" or "Christian," "Native" or "European." Most members of the society probably avoided such categorizations. This certainly was so for most government servants, European and Indian. Indeed, Brahmans were conspicuously silent. (Numerically dominant Smarthas, as Saivas so confident in their own immutable and cosmic superiority, may never have felt threatened. Sri-Vaishnavas, so long ago themselves converts and so highly exclusive, may have been among the most offended.)

Perceptions of the role of government could vary. Was it aloof ("neutral")—as it sometimes claimed in matters of religion—or active ("interfering")? Was it ambivalent—a weather vane turning with each prevailing current of opinion (hence, weak, indecisive, without any will of its own)? Or was it a grand arbitrator between extremely intolerant sets of contending forces (and hence, paternal and benevolently condescending to "keep the peace")? Most of all, was it simply cold and detached, never allowing the logic of its own position and power to be undermined? If so, no extremist faction could be allowed to gain too much influence, at least for long. That would have been dangerous to long-term political stability.[101]

Yet, in all this, one can clearly see a thread of consistency: both sides received substantial concessions. Moreover, this thread had at least two strands. First, the need of more loyal, higher-principled, and better-trained Indian officials required that, in the long run, support had to be given for a common higher learning, and this, by common elite consensus, had to be in English. Common ground rules were also necessary: impartial and scientific assessment of merit as the best basis for recruitment, promotion, and pension rather than hereditary connection, jobbing, and favoritism. Such a system was what both Munro and Norton wanted. Promotion by merit survived as fixed policy. This in time was seen as the single great concession to the conservative gentry (in the city if not in the country). Indeed, for a while, Madras became known as the "Brahman Raj."

Second, the instilling of high moral principles was left to "private" agencies. Inevitably and invariably, if moral reforms touched religion and if moral reforms touched customs, certain dilemmas and dangers were inescapable. It became necessary for the Company, like Janus, to face two ways at once. On one side, its authority required attachment to the religious symbols of the land—ceremonies, deities, festivals, temples, and such. Its *puja*—the perceived magic, power, and worship of its authority—had to be strong. Yet, on the other side, a further amalgamation of religious segments and a stronger process of integration would involve, at the very least, more interference. Some sensitive issues of institutional life, hallowed in custom and religion, would be touched. If left in "private" hands—

[100] "Humble Petition of Peroomoolum Ramasawmy Pillay, Agent of the Hindoos of Streenycondum Talook in the Zillah of Tirnavelly," January 3, 1846, MJC, BC, 114.783.

[101] Louis Fred Knoll, "State and Religion in British India: 1814–1865" (Ph.D. dissertation, Graduate Theological Union, Berkeley, Calif., 1971).

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to missionaries and reformers—government might henceforth evade the onus of interfering.

It was precisely at this point that the Tweeddale government stumbled. By Regulation VII of 1817, all religious institutions had come under state management and protection. The proper functioning of all temples and the proper worship (*puja*) of their deities were protected by government. Government itself, as the leading participant and upholder of corporate *puja*, was responsible. Efforts to convert people, even while entirely disconnected from official actions, also had to come under this protection. Direct efforts by one district officer to disparage Hindu institutions and to convert people under his charge had provoked a ferocious reaction from Munro:

> If he had been a missionary, what more could [Dalzell] have done? He did not, it is true, use any direct compulsion. That would most probably have caused an explosion, which would instantly have roused him from his delusion. But he did and will continue to use, unknown to himself, something very like compulsion—open interference, official agency, the hope of favour, the fear of displeasure. . . . There can be no real freedom of choice where official authority is interested deeply and exerted openly.[102]

To Munro, nothing was more dangerous than rulers who, when so few and so different, tampered with religious feelings. Such feelings could lie dormant and then, when least suspected, "burst forth in the most tremendous manner, as at Vellore."[103]

That the Court of Directors still shared this view became clear in 1847. "Deep concern" at the tendency "by all parties to give their disputes a religious character" turned to censure. Such attitudes were "incompatible with the calm and impartial administration of justice" and could not "too strongly be deprecated." Worse yet, and "entirely new, [was] use of the term '*heathen*' to signify the people of India."

> As applied to Hindoos, or to Hindoos and Mohomedans conjointly, it cannot but be felt as an approbious epithet. We consider it to be repugnant to that regard for the feelings of the people which forms an essential part of genuine toleration. We should, therefore, have expected that the phrase "heathen" would have been censured by Government . . . ; and we have observed with equal surprise and disapprobation that the phrase is adopted in the proceedings of the Government itself.[104]

The duty of government and its officers was to stay aloof from all religious activity, neither promoting nor opposing it.

Rebukes of increasing severity came from London (until Tweeddale was gone). The old curriculum of Madras University had already served "the moral education of the Natives." It was "neither expedient nor prudent" to put the Bible into "national schools." Tweeddale's Council of Education—"more likely to embarass than advance the cause of education"—had to go and the university board be

[102] Arbuthnot, *Selections*, 2: 300, 298–304. Dalzell was summarily dismissed.
[103] *Ibid.* Also see TNSA, MPC, no. 26, November 8, 1836, 657: 5403–58.
[104] COD to GOM (Judicial Department), no. 1, January 20, 1847, "Papers Relating to the Removal of Mr. Malcolm Lewin . . . ," Appendix 11 to Report of Select Committee on Indian Territories, *PP* (H. C., 1852–53), XXVII, p. 558.

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immediately restored. Henceforth, "superior education for the higher orders" would be the main sphere of direct government involvement in education. A final rebuke (December 1, 1847) reveals how little London liked religious disputes. The Court of Directors deplored

> urgent representations of a general feeling of alarm in the minds of Natives that the Government and its officers entertain hostile designs against their religion and give countenance and support to Missionaries in prosecution of such designs [however much explained by] assurance that representations of such a nature [are] groundless [and] that the only semblance of Native excitement and discontent proceed[s] from the Artifices of a few ill-disposed individuals, chiefly Europeans, and from unfair or untrue Statements in the newspapers.[105]

Madras officials should never have allowed such sensitive discussions to get out of hand. Acrimony in open court, in the public press, and in council chambers (revealed through leaked documents) had embarrassed London. Government had failed "to avoid giving cause for alarm to the Natives on the score of religion, whether by encroaching on their established rights and usages, or by treating [them] with neglect or contempt." There could be no room for ambiguity in such matters.[106]

Thus did modern education finally become established. Despite difficulties during the Pottinger administration and the "Grant-in-Aid" policy of Sir Charles Wood, the policy of providing advanced modern education for the social elite remained firm. Madras University became the major recruiting ground for successive generations of ever more highly trained officials. This exclusive and select leadership, almost entirely "clean-caste" and mainly Brahman, held sway within the imperial administration and within princely governments in the south. The position of this mandarin class was never seriously challenged until well into the twentieth century.[107]

[105] Despatch, COD to GOM (Public Department), no. 4, December 1, 1847, par. 4, MJC, BC, 112.092; and "Complaints of the Natives of India on the Subject of Religion," March 28, 1848, MJC, BC, 113.374.

[106] Despatch, COD to GOM (Public Department), no. 4, December 1, 1847, pars. 6–7, MJC, BC, 112.092.

[107] A final caveat must close this study. Many may wonder, with no small shock, how it is possible that an essay on early modern education in India could be written that does not invoke the names of Lord William Bentinck, Sir Charles Trevelyan, and Lord Thomas Babington Macaulay, much less Jeremy Bentham, James and John Stuart Mill, David Ricardo and Thomas Malthus, as well as others who were notable for contributions to education in India. No mention of "Macaulay's Minute"? No mention of Raja Ram Mohan Roy? These are part of the history of modern education within the context of Bengal. The linkages between education in Bengal and Madras and Bombay must wait for another essay.

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Turner, the Boltonians, and the Borderlands

DAVID J. WEBER

FOR MUCH OF THIS CENTURY, Frederick Jackson Turner's frontier thesis has been regarded as a most useful, if not the most useful, concept for understanding the distinctive features of American civilization. The existence of a frontier, Turner argued, explained much of the difference between Europe and the New World. As he put it in the famous paper that he delivered to the American Historical Association in 1893, "The peculiarity of American institutions is the fact that they have been compelled to adapt themselves to the changes of an expanding people—to the changes involved in crossing a continent, in winning a wilderness, and in developing . . . the primitive economic and political conditions of the frontier into the complexity of city life."[1] Turner suggested that circumstances peculiar to the American frontier, such as free land, opportunity, and common danger from Indians, shaped American character and institutions in specific ways: the frontier quickened assimilation of immigrants, had a "consolidating" and "nationalizing" effect on young America, and promoted democracy.[2] Moreover, Turner wrote, "to the frontier the American intellect owes its striking characteristics": inventiveness, practicality, inquisitiveness, restlessness, optimism, and individualism.[3]

This overarching explanation of American history rejected the conventional wisdom that American institutions and character had been transplanted unchanged from Europe. As Ray Allen Billington, the foremost explicator of Turner's ideas, once wrote, Turner "shook the academic world to its foundations."[4] Radical as it was, Turner's frontier thesis came to enjoy widespread acceptance, spawning a remarkable series of books and articles before coming under attack in the 1930s and 1940s. Much of the criticism was well-founded.

I wish to thank Allan G. Bogue, William Cronon, Donald C. Cutter, Lawrence Kinnaird, Patricia Nelson Limerick, and Donald Worcester for commenting on an earlier draft of this manuscript, which I read in a shorter version at the annual meeting of the Organization of American Historians held in Minneapolis in April 1985.

[1] Turner, "The Significance of the Frontier in American History," in George Rogers Taylor, ed., *The Turner Thesis Concerning the Role of the Frontier in American History* (3d edn., Lexington, Mass., 1972), 3. Turner's essay is available in a number of sources.

[2] *Ibid.*, 12, 17, 22.

[3] *Ibid.*, 27.

[4] Billington, *The Genesis of the Frontier Thesis: A Study in Historical Creativity* (San Marino, Calif., 1971), 3.

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Turner had overstated his case and failed to define terms carefully. He even used the term "frontier" imprecisely, sometimes to represent a place, sometimes a process, and sometimes a condition (a looseness of expression that I myself have failed to avoid in this essay). By the 1950s, however, Turner's defenders had clarified and refined his thesis in a convincing fashion. In modified form, it remains yet today a useful model for many historians.[5]

SINCE TURNER OFFERED A CONVINCING EXPLANATION of how the New World came to differ from the Old,[6] it might be supposed that the thesis would have been embraced by scholars who have studied the northward advance of the Mexican frontier into the area of the present-day United States, as well as by those who have interested themselves in the Anglo-American westward movement. In general, that has not been the case. Nearly all of Turner's students ignored racial and ethnic minorities in the West, Hispanics and the Hispanic frontier not excepted.[7] Moreover, scholars who specialize in the Hispanic frontiers of North America—the shifting region that has come to be called the Spanish Borderlands—have largely ignored Turner.[8] Some of his ideas have been fruitfully applied to the Borderlands by scholars working out of traditions unrelated to the Borderlands "school," but his thesis has made little impact on the historiography of the Borderlands or of Mexico itself. Yet Herbert Eugene Bolton, the founder of the Borderlands school, recognized at an early date the wisdom of applying Turner's thesis to Spanish-American frontiers.

Bolton received his doctorate in medieval history from the University of Pennsylvania but earlier studied American history under Turner at Wisconsin. As an undergraduate, Bolton took a course from Turner in 1895, less than two years after Turner delivered his famous address. Bolton stayed on at Wisconsin to do a master's degree under Turner and for the rest of his career was proud to have

[5] The best study of the reception of Turner's ideas is by Billington. See his *The American Frontier Thesis: Attack and Defense* (Washington, D.C., 1971). The most sophisticated defense of Turner is Billington's *America's Frontier Heritage* (New York, 1966). For recent assertions of the general validity of Turner's thesis, see Margaret Walsh, *The American Frontier Revisited* (Atlantic Highlands, N.J., 1981), 15, 71; and John Barker, *The Super Historians: Makers of Our Past* (New York, 1982), 346.

[6] As Turner put it, "The advance of the frontier has meant a steady movement away from Europe"; "Significance of the Frontier," 5.

[7] For a discussion of how Turner and his students ignored ethnic minorities, see Frederick C. Luebke, "Ethnic Minorities in the American West," in Michael P. Malone, ed., *Historians and the American West* (Lincoln, Nebr., 1983), 387–404. Luebke wrote, "Turner provided the key to ethnic history in his methodology, but his students fumbled at the door"; *ibid.*, 403. An important exception was Arthur Preston Whitaker, who worked under Turner at Harvard. See Whitaker, *The Spanish-American Frontier, 1783–1795: The Westward Movement and the Spanish Retreat in the Mississippi Valley* (1927; reprint edn., Lincoln, Nebr., 1969). Whitaker dedicated his book to Turner. He did not, however, attempt to apply the Turner thesis to the Spanish frontier but focused instead on diplomacy and politics.

[8] Although the area examined by the scholars of the Bolton school of Spanish Borderlands historiography extends from California to Florida, this essay focuses on the western Borderlands, from California to Texas, and on the area that lies above the present U.S.-Mexican border. This region was Bolton's major interest and is the area that has attracted the most attention from Borderlands scholars. Moreover, to treat both the eastern and western Borderlands in a single brief essay would be unwieldy. See Donald C. Cutter, "The Western Spanish Borderlands," in Malone, *Historians and the American West*, 40–41.

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been, as he put it, one of Turner's "boys."[9] In 1911 Bolton took a position at the University of California—a job that he obtained with Turner's help. That year Bolton told Berkeley's president, Benjamin Ide Wheeler, that, although Turner had directed the attention of researchers toward the West, he had "not thus far reached beyond the Mississippi Valley." Historians at Berkeley, Bolton suggested to Wheeler, should extend Turner's work by studying the Southwest and the Far West, particularly Spanish activities in the area that is now the United States.[10] Bolton did not simply want to study what Turner and his disciples had neglected; he also saw the need to analyze the Hispanic frontier as Turner had the Anglo-American frontier. "For him who interprets, with Turner's insight, the methods and the significance of the Spanish-American frontier," Bolton wrote in 1917, "there awaits a recognition not less marked or less deserved."[11]

Bolton continued to play this refrain. In 1932, in his own address to the American Historical Association, "The Epic of Greater America," he harkened back to the Turner thesis. Bolton asked, "Who has tried to state the significance of the frontier in terms of the Americas?"[12] In a seminar in Mexico City in 1946, Bolton sang the praises of Turner's "epic" essay to his students and then urged them to consider "to what extent his thesis is applicable or inapplicable to Mexico. . . . Perhaps the scholar who some day will discover and formulate such a thesis sits before me. Who can tell?"[13] No such student sat before Bolton that day or any other day. Bolton pointed the way, but he did not lead by example. In his own writing, he rarely attempted to apply to the Hispanic frontier the questions raised by Turner.[14]

Bolton's published work suggests that he was far more interested in the impact of Spaniards on the frontier than in the influence of the frontier on Spaniards. Heroic figures and the high drama of exploration and international rivalry captivated him, and the establishment of Spanish institutions in the Borderlands interested him intensely. Bolton clearly saw the mission and the presidio as "characteristically and designedly frontier institutions," but he saw them as extending, holding, and "civilizing" the frontier.[15] How frontier conditions might have altered these institutions or their inhabitants seldom concerned him.

[9] John Francis Bannon, *Herbert Eugene Bolton: The Historian and the Man, 1870–1953* (Tucson, Ariz., 1978), 14–15, 21.

[10] Report from Bolton to Wheeler, December 18, 1911, in John Francis Bannon, ed., *Bolton and the Spanish Borderlands* (Norman, Okla., 1964), 25.

[11] Bolton, "The Mission as a Frontier Institution in the Spanish American Colonies," in Bannon, *Bolton and the Spanish Borderlands*, 189. Bolton presented this essay as a faculty research lecture at the University of California in March 1917; it was reprinted widely.

[12] Bolton, "The Epic of Greater America," in Bannon, *Bolton and the Spanish Borderlands*, 332. This essay was first published in the *American Historical Review*; *AHR*, 38 (1932–33): 448–74.

[13] Bolton, "The Northward Movement in New Spain," in Bannon, *Bolton and the Spanish Borderlands*, 68. Also see *ibid.*, 78. This essay began as an outline for the seminar in Mexico City.

[14] For a recent analysis of Bolton's work and that of his followers, see Cutter, "Western Spanish Borderlands," 39–56. For recent historiographical essays on more specific topics, see Ellwyn R. Stoddard *et al.*, eds., *Borderlands Sourcebook: A Guide to the Literature on Northern Mexico and the American Southwest* (Norman, Okla., 1983). Included are two essays by Ralph H. Vigil, "Exploration and Conquest" and "Colonial Institutions." The index to this large reference work cites Turner only twice, although the volume does include an interesting review of theoretical literature by anthropologist Paul Kutsche, entitled "Borders and Frontiers."

[15] Bolton, "Mission as a Frontier Institution," 192, 199.

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In his engaging survey, *The Spanish Borderlands*, published in the Chronicles of America series in 1921, Bolton denied by implication that the frontier influenced Hispanic institutions and character. In Bolton's view, Spanish absolutism reached to all corners of the New World under the Habsburgs and stifled initiative, individual liberty, and self-government.[16] Because of the pervasive influence of their government, Bolton wrote, Spaniards "attained to little greater degree of personal freedom and little larger share in their own government in a frontier presidio than in the City of Mexico or in Seville."[17] One exception was the remote province of Alta California. On that frontier, Bolton explained, a peculiar set of conditions—the benign climate, unusually fertile soil, the abundance of Indian labor, the paternalism of the Spanish monarchy, and the area's distance from markets—conspired to dull initiative and produce an "idle" group of Hispanic frontiersmen.[18] Idle they may have been, but, Bolton suggested, the Californios were superior to other Mexicans because they were isolated from the rest of Mexico and had "the greater degree of independence, social at least if not political," that distance afforded them. Even at that early date, Bolton thought that he detected a "mellower spirit" in California.[19]

Bolton's students followed his example rather than his admonitions. None explicitly applied the Turner thesis to Spanish-American frontiers. Of course, some of the 104 Ph.D.'s whom Bolton trained employed the term "frontier" in the titles of their books, and many made notable contributions to our knowledge of the Hispanic frontier in North America. In the process, the Bolton school offered a valuable balance to the chauvinism of the Turnerians, who had come to see the term "frontier" as synonymous with the Anglo-American frontier. Some second-generation Boltonians recognized that frontiers had two sides and looked searchingly at interactions along Euro-Indian frontiers.[20] But Bolton's extraordinary academic progeny concerned itself more with archival research and the reconstruction of the particulars of the past than with theory in general or with the impact of the frontier on Mexican society or institutions in particular.[21]

[16] Bolton, *The Spanish Borderlands: A Chronicle of Old Florida and the Southwest* (New Haven, Conn., 1921), 233–34.

[17] *Ibid.*, 289.

[18] *Ibid.*, 290–91, 293.

[19] *Ibid.*, 294.

[20] See Jack D. Forbes, "Frontiers in American History," *Journal of the West*, 1 (1962): 63–73.

[21] Bannon listed all of the recipients of M.A.'s and Ph.D.'s whom Bolton directed; *Herbert Eugene Bolton*, 283–90. Bolton himself used the term "frontier" in his famous essay "Mission as a Frontier Institution" and in his *Athanase de Mezières and the Louisiana-Texas Frontier, 1768–1780* (Cleveland, Ohio, 1914). Also see Lawrence Kinnaird, ed., *The Frontiers of New Spain: Nicolás de Lafora's Description, 1766–1768* (Berkeley and Los Angeles, 1958); Alfred Barnaby Thomas, ed. and trans., *Forgotten Frontiers: A Study of the Spanish Indian Policy of Don Juan Bautista de Anza, Governor of New Mexico, 1777–1787; From Original Documents in the Archives of Spain, Mexico, and New Mexico* (Norman, Okla., 1932); and Max L. Moorhead, *The Apache Frontier: Jacobo de Ugarte and Spanish-Indian Relations in Northern New Spain, 1769–1791* (Norman, Okla., 1968). A sample of the scholarship of some of Bolton's students, characterized by works that are highly particular and consist in many cases of edited documents, appears in a Festschrift published in 1932, by which time Bolton had trained over fifty Ph.D.'s. See *New Spain and the Anglo-American West: Historical Contributions Presented to Herbert Eugene Bolton*, 2 vols. (Los Angeles, 1932). Also see *Greater America: Essays in Honor of Herbert Eugene Bolton* (Berkeley, 1945). This collection is another Festschrift and contains "A Bibliography of the Historical Writings of the Students of Herbert Eugene Bolton." I do not mean to suggest that all of the Boltonians

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Nowhere was the scanty interest of the Borderlands school in the Turner thesis better illustrated than in John Francis Bannon's *The Spanish Borderlands Frontier.* Published in 1970, Bannon's study was the first effort to synthesize the burgeoning literature on the Spanish-American frontier in what is today the United States. Bannon's survey appeared in the Histories of the American Frontier series, edited by Ray Allen Billington, Turner's most distinguished disciple. Nonetheless, Bannon's *Spanish Borderlands Frontier* contains no mention of Turner or his thesis.

Like Bolton, Bannon implicitly rejected the idea that the frontier democratized Hispanic institutions. "The shadow of Spanish absolutism," he wrote, "allowed the Spanish frontiersman little chance to develop a sense of self-expression or a feeling of self-reliance."[22] Although a small body of scholarship suggested other possibilities, Bannon concluded that Hispanic, in contrast to Anglo-American, frontiersmen lacked self-determination and the personal liberty to move freely over space. "Rugged individualism was foreign to the Borderlands," he concluded.[23] Bannon did suggest, however, that the physical environment on the frontier shaped Spanish economic life and institutions. The "rugged, arid, and often sterile" terrain encouraged ranching and mining rather than agriculture.[24] Moreover, hostile Indians (along what Turner called "the meeting point between savagery and civilization") provided the reason for the existence of two major Spanish frontier institutions—the presidio and the mission.[25]

In the decade and a half since Bannon's synthesis appeared, Borderlands historians of the Bolton school have continued to investigate many of the themes that were of interest to Bolton and, with few exceptions, to ignore Turner. Borderlands specialists have written fine books examining such traditional subjects as international rivalries, Spanish-Indian relations, the lives of prominent persons, and the presidio and the mission. If Turner's ideas influenced these scholars, their work does not reveal it.[26] Historians following two other modes of inquiry, however, have used Turner's ideas in ways that have deepened our understanding of the Borderlands.

did narrow work based on archival research. Many of them also wrote wide-ranging, interpretive essays. See, for example, Norris Hundley, Jr., and John A. Schutz, eds., *The American West: Frontier and Region; Interpretations by John Walton Caughey* (Los Angeles, 1969).

[22] Bannon, *The Spanish Borderlands Frontier, 1513–1821* (New York, 1970), 5.

[23] *Ibid.*, 6, 237–38.

[24] *Ibid.*, 233–34.

[25] *Ibid.*, 234; and Turner, "Significance of the Frontier," 4.

[26] See, for example, Warren L. Cook, *Flood Tide of Empire: Spain and the Pacific Northwest, 1543–1819* (New Haven, Conn., 1973); Abraham P. Nasatir, *Borderland in Retreat: From Spanish Louisiana to the Far Southwest* (Albuquerque, N.Mex., 1976); Elizabeth A. H. John, *Storms Brewed in Other Men's Worlds: The Confrontation of Indians, Spanish, and French in the Southwest, 1540–1795* (College Station, Tex., 1975). For biography, see, for example, Felix D. Almaraz, Jr., *Tragic Cavalier: Governor Manuel de Salcedo of Texas, 1808–1813* (Austin, Tex., 1971); Edwin A. Beilharz, *Felipe de Neve: First Governor of California* (San Francisco, 1971); and Francis F. Guest, *Fermín Francisco de Lasuén (1736–1803): A Biography* (Washington, D.C., 1973). On institutions, see Max L. Moorhead, *The Presidio: Bastion of the Spanish Borderlands* (Norman, Okla., 1975); John L. Kessell, *Mission of Sorrows: Jesuit Guevavi and the Pimas, 1691–1767* (Tucson, Ariz., 1970), *Friars, Soldiers, and Reformers: Hispanic Arizona and the Sonora Mission Frontier, 1767–1856* (Tucson, Ariz., 1976), and *Kiva, Cross, and Crown: The Pecos Indians and New Mexico, 1540–1840* (Washington, D.C., 1979).

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ONE GROUP CONSISTS of those who have attempted comparative analyses. Some of these scholars began in the 1920s, if not before, to apply the Turner thesis to frontiers throughout Spanish America.[27] Although these writers would disagree among themselves on many points, their collective effort suggests several conclusions that apply to the Spanish-American frontiers that pushed into areas now in the American West.

First, these historians noted that the physical environments of frontiers differ, so the impacts of frontier environments on cultures also differ. The distinguished Peruvian historian Victor Andrés Belaúnde pointed out, in an essay published in English in 1923, that the mere existence of plentiful free land, the key ingredient on the American frontier according to Turner, was not sufficient in itself to shape institutions and culture. Quality of land was more important than quantity. Land had to be accessible and capable of productivity, but much of Latin America is tropical rain forest or marginal sierra or altiplano, possessing little of what Belaúnde called "human value." Because they could not effectively exploit these marginal frontier lands, he argued, some Latin American nations "lack . . . the characteristics of frontier countries."[28] Turner, who claimed that the American frontier was unique, might not have disagreed. After all, he had argued that "Western democracy . . . came stark and strong and full of life, from the American forest" and would hardly have expected the Amazon jungle or the arid frontiers of northern Mexico to make the same impact on society that the more hospitable woodlands of the Old Northwest did.[29]

Second, in their comparative studies these scholars have moved well beyond the simple notion of the frontier as a line between "savagery and civilization" to remind

[27] I refer here deliberately to Spanish America rather than to Latin America in order not to confuse Portuguese America with Spanish America and to exempt the considerable literature on Brazilian frontiers from the discussion. For a bibliography on this subject and an often insightful if disjointed essay comparing Latin American and Anglo-American frontiers, see Alistair Hennessy, *The Frontier in Latin American History* (Albuquerque, N.Mex., 1978). Some of the most sophisticated works in frontier history have been written on Brazilian frontiers. See, especially, Martin T. Katzman, "The Brazilian Frontier in Comparative Perspective," *Comparative Studies in Society and History*, 17 (1975): 266–85. Since Hennessy's work appeared, students of Latin America have continued to probe the question of comparative frontiers. William Frederick Sharp concluded that "the classic waves of frontier development described by Frederick Jackson Turner did not occur in the Chocó." See his *Slavery on the Spanish Frontier: The Colombian Chocó, 1680–1810* (Norman, Okla., 1976), 3. Jane M. Rausch reached a similar but more guarded conclusion. See Rausch, *A Tropical Plains Frontier: The Llanos of Colombia, 1531–1831* (Albuquerque, N.Mex., 1984), 230, 245. The works of other scholars who have attempted to make broad comparisons of Anglo-American and Spanish-American frontiers are cited below. In addition, see Seymour Martin Lipsett, "The Newness of the New Nation," in C. Vann Woodward, ed., *The Comparative Approach to American History* (New York, 1968), 70–71; and C. H. Haring, *The Spanish Empire in America* (New York, 1947), 30–41. Haring employed an implicitly Turnerian approach to an analysis of the impact of the New World environment on the English, Portuguese, and Spanish frontiers. Also see Dietrich Gerhard, "The Frontier in Comparative View," *Comparative Studies in Society and History*, 1 (1959): 205–29. Gerhard, in this landmark essay, noted that "Latin America would be another appropriate area for comparison. I do not feel, however, competent to deal with it" (p. 206).

[28] Belaúnde, *The Frontier in Hispanic America*, Rice Institute Pamphlets, no. 10 (Houston, Tex., 1923), 208. Other writers would agree with Belaúnde. See, for example, Donald J. Lehmer, "The Second Frontier: The Spanish," in Robert G. Ferris, ed., *The American West: An Appraisal* (Santa Fe, N.Mex., 1963), 143–44.

[29] Turner, as quoted in Rex W. Strickland, *The Turner Thesis and the Dry World* (El Paso, Tex., 1964), 16. Strickland saw the Turner thesis as inapplicable to the Rio Grande Valley of New Mexico and West Texas.

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us that a variety of indigenous societies can exist in a frontier zone and that different host societies have different impacts on the cultures and institutions of intruders. Hence, an understanding of the frontier process must take into account peoples and their motives on both sides of a frontier. Spanish-Americans, comparative historians tell us, attempted to assimilate indigenous Americans rather than push them back or annihilate them as the English generally did. Moreover, Spaniards often encountered indigenous peoples whose culture rendered them easily assimilable. The Spanish-American frontier, in the words of geographer Marvin Mikesell, was a "frontier of inclusion" in contrast to the Anglo-American "frontier of exclusion."[30]

Third, these historians have also moved beyond the simple environmentalism expressed in Turner's famous essay.[31] Several of those who have compared Latin American and Anglo-American frontiers have concluded that the culture pioneers brought to the frontier was far more important than the impact of the frontier on that culture.[32] In advancing this argument, Billington has nearly turned Turner's thesis on its head. He has suggested that Turner did not intend to explain all of American history as shaped by a single force. Notwithstanding Turner's unequivocal assertion that "free land, its continuous recession, and the advance of American settlement westward, explain American development,"[33] Billington concluded that the frontier could *not* "affect *major* changes in either the personalities or the behavioral patterns of frontiersmen. . . . The bulk of the customs and beliefs of the pioneers were transmitted, and were only slightly modified by the changing culture in which they lived. . . . Men, not geography, explain the differences between the Anglo-American and Latin-American frontiers, for individuals of different backgrounds will respond in different ways to identical physical environments."[34] Ironically, those historians of comparative frontiers who have modified and reinterpreted Turner's thesis have moved closer to Bolton's

[30] Mikesell, "Comparative Studies in Frontier History," *Annals of the Association of American Geographers*, 50 (1960): 65. Also see Hennessy, *Frontiers in Latin American History*, 19–20; and Forbes, "Frontiers in American History," 65, and "Frontiers in American History and the Role of the Frontier Historian," *Ethnohistory*, 15 (1968): 205; T. M. Pearce, "The 'Other' Frontiers of the American West," *Arizona and the West*, 4 (1962): 105–12; and Owen Lattimore, "The Frontier in History," in Robert A. Manners and David Kaplan, eds., *Theory in Anthropology* (Chicago, 1968), 375. For a recent summation, see Howard Lamar and Leonard Thompson, eds., *The Frontier in History: North America and Southern Africa Compared* (New Haven, Conn., 1981).

[31] Billington made a convincing case that Turner was not simply an environmentalist or a monocausationist, but that is not apparent in Turner's famous and influential essay. See Billington, *The American Frontier Thesis*, 13–15.

[32] Even Belaúnde, who made a strong case for environmental differences, used politics and culture to explain why the frontier failed to provide opportunity in those areas where Latin America's geography resembled that of the United States; *The Frontier in Hispanic America*, 212–13. Also see Lehmer, "The Second Frontier," 144–50; and William H. Lyon, "The Third Generation and the Frontier Hypothesis," *Arizona and the West*, 4 (1962): 48.

[33] Turner, "Significance of the Frontier," 3.

[34] Billington, *America's Frontier Heritage*, 54. (Emphasis added.) Billington expressed similar sentiments in three other essays: "The Frontier in American Thought and Character," in Archibald B. Lewis and Thomas F. McGann, eds., *The New World Looks at Its History* (Austin, Tex., 1963), 78–79; "Frontiers," in Woodward, *The Comparative Approach to American History*, 77–78; and "Turner and Webb," in Harold M. Hollingsworth and Sandra L. Myres, eds., *Essays on the American West* (Austin, Tex., 1969), 89.

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position. In his own work, and in that of many of his students, Bolton focused on the interplay of cultures on both sides of the frontier, be it Spanish and Indian or Spanish and French. Bolton also stressed the importance of the institutions and the culture of the individuals that the invading country brought to a frontier. Nonetheless, with only a few exceptions, historians working in the Bolton tradition have paid scant attention to how comparative studies of frontiers might shed light on our understanding of the Borderlands.[35] There is additional irony here in that Bolton was an early and passionate advocate of comparative analysis in the study of hemispheric history.

A few other scholars, however, whose training was not in the Bolton tradition, have taken up the challenge of applying Turner's ideas explicitly to Mexico's far northern frontiers. Their conclusions have varied. The least sophisticated inquiry is a study by Rex Strickland, who argued unequivocally that the Spanish-American frontier "did not produce even a semblance of democracy."[36] Silvio Zavala, the eminent Mexican historian, offered a more measured assessment.[37] Turner's thesis, Zavala believed, might apply to the northern frontier of Mexico—part of which now lies in the present United States. Citing Alexander von Humboldt approvingly, Zavala suggested that the insecurity of life in the Mexican north and

[35] Bannon, for example, relied solely on Lehmer's "The Second Frontier" in his study of the Spanish Borderlands. For other exceptions, see Forbes, "Frontiers in American History"; Oakah L. Jones, Jr., *Los Paisanos: Spanish Settlers on the Northern Frontier of New Spain* (Norman, Okla., 1979); and David J. Weber, *The Mexican Frontier, 1821–1846: The American Southwest under Mexico* (Albuquerque, N.Mex., 1982). Also see Sandra L. Myres, "The Ranching Frontier," in Hollingsworth and Myres, *Essays on the American West*, 33; and David J. Weber, ed., *Foreigners in Their Native Land: Historical Roots of the Mexican Americans* (Albuquerque, N.Mex., 1973), 19–21. Jones is a second-generation Bolton student, having studied with Max L. Moorhead, a student of Bolton's. Forbes and I are third-generation Boltonians, having studied with Donald C. Cutter, who studied with Bolton but finished his work under Lawrence Kinnaird, another student of Bolton's. Myres studied under Donald Worcester, who completed his M.A. under Bolton's supervision and his Ph.D. under Kinnaird. In speaking of a third generation of Boltonians, however, I may be pushing the idea of Bolton's influence and "school" too far.

[36] Strickland, *Turner Thesis*, 8. Strickland received his Ph.D. from the University of Texas, where he worked under Walter Prescott Webb. Webb argued that Spanish institutions did not adapt to the environmental challenge of the Great Plains. Although Spaniards succeeded as explorers on the high plains, they failed as colonists. Unlike Strickland, Webb did not mention Turner or the frontier thesis and instead denied the direct influence of Turner on his thinking. It is clear, however, that Webb had read Turner's essay before writing his book. See Webb, *The Great Plains* (Boston, 1931), 85–139; and Gregory M. Tobin, *The Making of a History: Walter Prescott Webb and the Great Plains* (Austin, Tex., 1976), 110–11. Also see Necah Stewart Furman, *Walter Prescott Webb: His Life and Impact* (Albuquerque, N.Mex., 1976), 122. Two decades after the appearance of *The Great Plains*, Webb urged historians to examine the frontiers of Latin America in light of Turner's thesis; Webb, *The Great Frontier* (Austin, Tex., 1951), 411–12.

[37] In this respect, Zavala stands apart from his Mexican counterparts who, like American historians, have made little attempt to apply Turner's thesis to Mexico's frontiers. See, for example, María del Carmen Velázquez, *Colotlán: Doble frontera contra los bárbaros* (Mexico City, 1961), *Establecimiento y pérdida del septentrión de Nueva España* (Mexico City, 1974), and *El Marqués de Altamira y las Provincias Internas de Nueva España*. Also see César Sepúlveda, *La frontera norte de México: Historia, conflictos, 1672–1975* (Mexico City, 1976), and *Tres ensayos sobre la frontera septentrional de la Nueva España* (Mexico City, 1977). Spanish historians who have examined the Mexican frontier, such as Luis Navarro García and Mario Hernández Sánchez-Barba, have also ignored the Turner thesis. An important exception is Mexican historian Vito Alessio Robles. Although he did not cite Turner, Alessio Robles briefly made some of the same points on which Zavala later elaborated. Alessio Robles, "Las condiciones sociales en el norte de la Nueva España," *Memorias de la Academia Mexicana de la Historia*, 4 (1945): 156–57. American historians, not of the Bolton school, who have explicitly applied the Turner thesis to the Mexican frontier include Strickland and C. Alan Hutchinson.

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the unavailability of a docile Indian labor force "stamped the character of the northern people with a certain temper and energy."[38] Zavala also quoted a contemporary of von Humboldt, Miguel Ramos de Arizpe, who claimed that, compared to other Mexicans, northerners were more energetic, hard working, liberty loving, and "devoted to the liberal and mechanical arts." Mexico's northern frontier, Zavala concluded, "seemed to be the guardian of liberty," but he also recognized that the isolated region could become a refuge for political despots, whose power could not be curbed easily by the distant central government.[39]

Zavala's idea that distance could encourage despotism seems to apply especially well to Alta California, the most isolated province on the Mexican frontier. The point has been made forcibly by C. Alan Hutchinson, who explicitly tested Turner's thesis in Mexican California. An Englishman trained outside of the Bolton tradition, Hutchinson found that the region lacked two ingredients that were essential to Turner's interpretation: land and liberty. Missions or large estates occupied the choicest land in California, Hutchinson argued, and distance alone discouraged Mexicans from moving there. To explain why the California frontier failed to promote democracy and instead became a refuge for petty tyrants, Hutchinson spun Turner's argument around. "The frontier," he wrote, "reproduces, in somewhat more visible fashion, what is already present in the homeland from which the settlers came."[40] In this sense, the Turner thesis applies. Whereas the American frontier promoted democracy, the Mexican frontier promoted caudillismo, or petty despotism. Finally, Hutchinson argued that frontier California's abundant and exploitable supply of Indian laborers discouraged the Californios from working with their own hands. Thus, they failed to develop "such frontier virtues as independence or resourcefulness."[41] Perhaps because he based his conclusions on an examination of the neglected Mexican period (1821–46) rather than the preceding Spanish era, which has held greater interest for historians, Hutchinson's conclusions regarding the relationship between the frontier and political democracy have been largely ignored by Borderlands scholars.

[38] Zavala, "The Frontiers of Hispanic America," in Walker D. Wyman and Clifton B. Kroeber, *The Frontier in Perspective* (Madison, Wis., 1965), 48. For the larger essay in Spanish of which "Frontier of Hispanic America" is a part, see *Cuadernos Americanos*, 17 (1958): 374–84. For the original Spanish version of "Frontiers of Hispanic America" with more extensive documentation, see David J. Weber, ed., *El México perdido* (Mexico City, 1976), 150–66.

[39] Zavala, "Frontiers of Hispanic America," 49–50. Discussing frontiers generally in the period before 1750, William H. McNeill recently came to a similar conclusion. "Compulsion and legally reinforced forms of social hierarchy were more generally characteristic of frontier society than were equality and freedom"; McNeill, *The Great Frontier: Freedom and Hierarchy in Modern Times* (Princeton, N.J., 1983), 26. McNeill's explanation, however, differs from Zavala's yet also applies to some Hispanic frontiers. McNeill saw egalitarianism diminished by the need of frontier elites to control scarce labor and resources, the need to sustain armies for protection on frontiers, and dependency on outside markets.

[40] Hutchinson, *Frontier Settlement in Mexican California: The Híjar-Padrés Colony and Its Origins, 1769–1835* (New Haven, Conn., 1969), 398. For a reprint of Hutchinson's commentary on the Turner thesis, see David J. Weber, ed., *New Spain's Far Northern Frontier: Essays on Spain in the American West, 1540–1821*, (Albuquerque, N.Mex., 1979), 173–77, and *El México perdido*, 140–49. Hutchinson received his Ph.D. from the University of Texas.

[41] Hutchinson, *Frontier Settlement*, 399.

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AT THE SAME TIME that a number of scholars tested the frontier thesis in comparative studies, a group of social historians found some of Turner's ideas applicable to the Borderlands. In the main, the Bolton school demonstrated little interest in social history.[42] The few scholars who have concerned themselves with the nature of Borderlands society have generally come out of other traditions, and, with few exceptions, the most sophisticated studies have been written in the last two decades by historians who have adopted the quantitative methods and interdisciplinary modes of inquiry of the "new" social history. Many who have used these fresh approaches have focused on urban history, where Turner's thesis has little explanatory value.[43] Others, however, have written about the era of the shifting Spanish-American colonial frontier and have posed questions that would have intrigued Turner, who was himself a pioneer in interdisciplinary research and in the use of quantitative techniques.[44] Yet it seems clear that Turner's ideas have influenced these scholars only slightly; they seldom mention him explicitly. Instead, they acknowledge an intellectual debt to the Berkeley school of demography, to American social historians such as John Demos and Stephen Thernstrom, or to the French Annales school and focus on questions involving infant mortality, longevity, household size and composition, sex ratios, illegitimacy, and migration.[45]

Several practitioners of the new social history, however, have also commented on the impact of the frontier on Hispanic society. They have implicitly embraced Turner's notion that the frontier altered the character of Mexican frontiersmen in a variety of ways, such as promoting individualism and egalitarianism and providing what Turner called "conditions of social mobility."[46] The new social historians were not the first to make such suggestions, but they examined frontier society in greater detail and with more sophistication than their predecessors did.[47]

[42] See, for example, Antonio José Ríos-Bustamante, "A Contribution to the Historiography of the Greater Mexican North in the Eighteenth Century," *Aztlán*, 7 (1976): 350.

[43] For a discussion of some of this literature, see Roger W. Lotchin and David J. Weber, "The New Chicano Urban History," *History Teacher*, 16 (1983): 219–47.

[44] Billington, *The American Frontier Thesis*, 15–16.

[45] See, for example, James Michael McReynolds, "Family Life in a Borderland Community: Nacogdoches, Texas, 1779–1861" (Ph.D. dissertation, Texas Tech University, 1978). McReynolds modeled his dissertation after studies by John Demos. See Demos, *A Little Commonwealth: Family Life in Plymouth Colony* (New York, 1970). Also see Janie Louise Aragón, "The People of Santa Fe in the 1790s," *Aztlán*, 7 (1976): 391–417. Aragón did not look at frontier society in a broad context but probed census data to answer questions about population growth, occupations, and ethnic structure. For an examination of some of this literature in a broader context than I am able to provide in this brief essay, see Richard Griswold del Castillo, "Quantitative History in the American Southwest: A Survey and Critique," *Western Historical Quarterly*, 15 (1984): 407–26.

[46] Turner, "Contributions of the West to Democracy," in Ray Allen Billington, ed., *Selected Essays of Frederick Jackson Turner: Frontier and Section* (Englewood Cliffs, N.J., 1961), 95. Also see Gilberto Miguel Hinojosa, *A Borderlands Town in Transition: Laredo, 1755–1870* (College Station, Tex., 1983), 17–18, 43. Hinojosa found a high level of social stratification by ethnic group in Laredo, but he did not attempt to compare the frontier with the metropolis.

[47] See, for example, France V. Scholes, "Civil Government and Society in New Mexico in the Seventeenth Century," *New Mexico Historical Review*, 10 (1935): 97–98. Scholes argued that the frontier lessened social distinctions. (Scholes was not a Bolton student but studied with Turner at Harvard.) Alessio Robles suggested that the struggle against Indians, the physical environment, and the lack of Indian labor strengthened the character of colonists in the north, making them more energetic, farsighted, and tenacious; "Las condiciones sociales en el norte de la Nueva España," 156–57. François

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Alicia Tjarks has written the most ambitious and searching of these new studies. In an article entitled the "Urban Evolution of Texas," she explicitly mentioned the Turner thesis and argued that similarities existed between the American frontier and the proto-urban communities of early eighteenth-century Hispanic Texas. Tjarks found that the Hispanic pioneers in Texas exhibited "all of the features typical of the frontier." They were future-oriented, stoic, strong, lovers of the wild, and reluctant to accept official control.[48] Tjarks's study of late eighteenth-century census reports in Texas, in which she did not mention Turner, reveals an open society, less stratified than the society of central Mexico. Frontier Texas, she argued, represented a land of opportunity for Mexican immigrants in the late eighteenth century. Although political democracy did not flourish, upward social mobility occurred easily in this "melting pot of races."[49]

Tjarks also studied census returns from late eighteenth-century New Mexico. There, too, she found a racial melting pot but concluded that the process of racial blending occurred more slowly in New Mexico than in Texas. In the case of New Mexico, a considerable amount of racial amalgamation had occurred among Mexican immigrants even before they reached the province. Compared to her work on Texas, Tjarks's study of New Mexico is much narrower chronologically and less suggestive about the impact of the frontier on that society.[50] But another scholar, Antonio José Ríos-Bustamante, studied census data from late eighteenth-century Albuquerque and found that its frontier community possessed some of the characteristics that Turner noted on the American frontier (although neither Ríos-Bustamante nor Tjarks mentioned Turner in their studies of New Mexico). Albuquerque's citizens, according to Ríos-Bustamante, exhibited a higher degree of social mobility, a more rapidly growing rate of miscegenation, and greater egalitarianism than did the people who lived in central Mexico.[51]

Similarly, Leon Campbell did not mention Turner explicitly, but he explained the nature of society at the small military posts in Hispanic Alta California in Turnerian terms. Campbell concluded that in frontier garrisons "social distances between officers and enlisted men were greatly reduced" and upward social

Chevalier described the Mexican north (sixteenth- and seventeenth-century Nueva Galicia and Nueva Vizcaya in particular) as an area that allowed greater individual initiative and enterprise than did central Mexico, but he attributed this to distance from central authority rather than to frontier conditions; Chevalier, *Land and Society in Colonial Mexico: The Great Hacienda*, ed. Lesley Byrd Simpson and trans. Alvin Eustis (1952; reprint edn., Berkeley and Los Angeles, 1970), 148–51. For more recent assessments of the frontier as an area of relatively high social mobility, see Manuel Patricio Servín, "California's Hispanic Heritage: A View into the Spanish Myth," in Weber, *New Spain's Far Northern Frontier*, 117–33; and Ralph Vigil, "The Hispanic Heritage and the Borderlands," *Journal of San Diego History*, 19 (1973): 33, 38–39. For frontier individualism in New Mexico, see Marc Simmons, "Settlement Patterns and Village Plans in Colonial New Mexico," in Weber, *New Spain's Far Northern Frontier*, 97–115.

[48] Tjarks, "Evolución urbana de Texas durante el siglo XVIII," *Revista de Indias*, 131–38 (1973–74): 609.

[49] Tjarks, "Comparative Demographic Analysis of Texas, 1777–1793," *Southwestern Historical Quarterly*, 77 (1974): 322, 293–94.

[50] Tjarks, "Demographic, Ethnic, and Occupational Structure of New Mexico, 1790: The Census Report of 1790," *The Americas*, 35 (1978): 80.

[51] Ríos-Bustamente, "New Mexico in the Eighteenth Century: Life, Labor, and Trade in la Villa de San Felipe de Albuquerque, 1706–1790," *Aztlán*, 7 (1976): 379.

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mobility was easily achieved.[52] He saw California as a land of opportunity where "the widespread distribution of *land* . . . meant that California society was markedly less rigid than that of the metropolitan regions."[53] Isolated and enjoying a salutary neglect by Spanish officials, Californios "shared in the optimistic dream of the future largely associated only with the Anglo-Saxon culture."[54] Some of Campbell's conclusions stand in opposition to those of C. Alan Hutchinson, with whose work Campbell was apparently unfamiliar.[55]

In a major study of frontier society published at the end of the 1970s, Oakah L. Jones also took a Turnerian view of the frontier—without mentioning Turner. In *Los Paisanos: Spanish Settlers on the Northern Frontier of New Spain*, Jones examined the culture and institutions of Hispanic civilian settlers in the provinces along both sides of what became the U.S.-Mexican border. Frontier conditions, Jones concluded, altered Hispanic society dramatically. Although Hispanic frontiersmen depended on the government in the early stages of settlement, with the passage of time they "became more self-reliant, more individualistic, less class conscious, and more conservative in their political outlook than the people of central New Spain."[56] In general, Jones found that the harshness of frontier life had a leveling effect. In a phrase aimed perhaps at cliometricians, Jones noted that on the frontier "class rivalry and distinction had little place except for statistical purposes."[57]

Thus, although they have rarely cited Turner, Borderlands historians with an interest in social history have suggested in their recent works that Hispanic frontiersmen enjoyed greater opportunity for upward social mobility and lived in a more egalitarian society than their countrymen in the more settled areas of Mexico. Anthropologists, most notably Miguel León-Portilla and Frances León Swadesh, have come to similar conclusions.[58]

[52] Campbell, "The First Californios: Presidial Society in Spanish California," *Journal of the West*, 11 (1972), reprinted in Oakah L. Jones, Jr., *The Spanish Borderlands: A First Reader* (Los Angeles, 1974), 112, 115.

[53] *Ibid.*, 116. (Emphasis added.)

[54] *Ibid.* Campbell's article ranges across both the Spanish and Mexican periods without taking into account changes that might have occurred in California's institutions or economy with the advent of Mexico's independence from Spain in 1821.

[55] See Hutchinson, *Frontier Settlement.*

[56] Jones, *Los Paisanos*, 238, 252–53.

[57] *Ibid.*, 247.

[58] León-Portilla described the distinctive "ethos" of the residents of the present-day Mexican north, which he ascribed in part to the frontier. See León-Portilla, "The Norteño Variety of Mexican Culture: An Ethnohistorical Approach," in Edward H. Spicer and Raymond H. Thompson, eds., *Plural Society in the Southwest* (New York, 1972), 109–14. Swadesh examined a group of settlers in the Chama Valley of New Mexico and found that frontier conditions made them hard working, self-reliant, and independent. Their society was more fluid and egalitarian than the societies in more settled areas of New Mexico or Mexico, and land and trade with Indians provided opportunities for advancement. See Swadesh, *Los primeros pobladores: Hispanic Americans of the Ute Frontier* (South Bend, Ind., 1974), 17, 46, 60, 156–57, 159, 173. Henry F. Dobyns argued that the Hispanic frontier in Arizona did not promote individualism as Turner found on the American frontier. Instead, Dobyns suggested that Hispanics brought their own sense of individualism with them, and he surmised that "Hispanic individualism probably survived intact." Dobyns seemed to believe that the Hispanic frontier in Arizona promoted social mobility. He acknowledged that some soldiers achieved upward mobility at the Tucson presidio. His conclusion that "the population of this frontier post remained socially stratified along ethnic lines" must be tempered by his statement that miscegenation was well advanced in the presidio and that "army service function[ed] as one of those institutions of cultural contact between Spaniards and friendly Native American tribesmen which facilitated the acquisition of Hispanic traits by the latter." Dominated

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Ironically, although many of these studies have used quantitative techniques, the idea that frontier society was relatively open is based on impressionistic evidence. No scholar has made a statistical comparison between frontier societies and societies in more settled areas of central Mexico. Hence, claims to "relative" openness have only been asserted, not empirically demonstrated. Conventional wisdom—a wisdom that Turner helped render conventional—suggests, however, that this assertion is probably correct. Nonetheless, it is interesting to note that by the late eighteenth century racial and class distinctions had begun to break down throughout Mexico; for this period we have the most detailed census records and the largest number of studies on the northern frontier.[59]

IF MOST SOCIAL HISTORIANS AND ANTHROPOLOGISTS agree that the frontier altered the character of Mexican frontiersmen in ways that resemble Turner's description of changes among American frontiersmen, why has Turner's thesis failed to exert greater influence on Borderlands historiography or, for that matter, on Mexican historiography? Much of the explanation lies in understanding that the frontier never developed mythic importance in Mexican letters and popular culture and that Mexico's far northern frontier made little impact on Mexico's national culture and institutions. For Turner, "the *most important* effect of the frontier" was "the promotion of democracy here and in Europe."[60] But is there any evidence that the Mexican frontier was a cradle of Mexican democracy or the crucible of a Mexican national character? Some historians have answered that question affirmatively for the northern frontier of sixteenth-century Mexico. Commenting on Spanish expansion northward from Mexico City into the area of Zacatecas, Woodrow Borah characterized that region as a "melting pot" in which different races and ethnic groups "merged into a hybrid culture, clearly Hispanic but equally clearly a subtype—in other words, Mexican. The frontier rather than the center was the creator of Mexican culture and Mexican allegiance."[61]

Other writers have argued convincingly that residents of the present-day northern states of Mexico—Sonora, Sinaloa, Chihuahua, Durango, Coahuila, Nuevo León, and Tamaulipas—possess a unique character that has altered Mexican national life and politics.[62] No one, however, has made a case that the

by the church and the military, Tucson's society failed to develop a middle class, much less democracy. See Dobyns, *Spanish Colonial Tucson: A Demographic History* (Tucson, Ariz., 1976), 62, 65, 111–12. Neither Dobyns, León-Portilla, nor Swadesh cited Turner directly.

[59] See L. N. McAlister, "Social Structure and Social Change in New Spain," *Hispanic American Historical Review*, 43 (1963): 349–70.

[60] Turner, "Significance of the Frontier," 22. (Emphasis added.) Turner wrote a separate essay on "Contributions of the West to Democracy," published in the *Atlantic Monthly* in 1903. For a reprint, see Turner, *The Frontier in American History* (New York, 1920), 243–68; and Billington, *Selected Essays*, 77–97.

[61] Borah, "Discontinuity and Continuity in Mexican History," *Pacific Historical Review*, 48 (1979): 15. Philip Wayne Powell advanced a similar argument. See Powell, *Mexico's Miguel Caldera: The Taming of America's First Frontier, 1548–1597* (Tucson, Ariz., 1977), 262, and "North America's First Frontier, 1546–1603," in George Wolfskill and Stanley Palmer, eds., *Essays on Frontiers in World History* (Austin, Tex., 1981), 26. Zavala took the opposite view; "Frontiers of Hispanic America," 51.

[62] See, for example, Barry Carr, "The Peculiarities of the Mexican North, 1880–1928: An Essay in

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shifting Mexican-American frontier—those parts that are now within the United States (the border states from California to Texas)—ever made an impact on Mexican character and institutions. Some Mexicans hoped that it might liberate Mexico from its "semifeudal routine," as one writer put it in 1831, but Mexico's far northern frontier never exercised such influence. So long as it belonged to Spain or to Mexico, the region was too underpopulated, too peripheral, and too underdeveloped to influence activities in the nation's core.[63] Indeed, in the years before the Mexican-American War, the cutting edge of Mexico's northern frontier was not the area of most rapid and effective "Mexicanization," to paraphrase Turner. Nor did Mexicans generally regard the arid, hostile, and remote world of the shifting frontier as a "garden" that would lead to the constant rebirth or regeneration of Mexican culture—an idea that Henry Nash Smith has shown was central to the Anglo-American ethos. The Anglo-American frontier may or may not have promoted democracy, as Turner argued, but, because Americans widely believe that it did, the idea itself is of considerable importance. In Mexico, however, there has been no counterpart to American idealization of frontier life. No myth about the salubrious impact of the frontier exists on which a Mexican Turner might construct a credible intellectual edifice.[64]

Silvio Zavala appears to have been correct in arguing that the far northern frontier was not "the source of the Mexican national type." In Mexico, the greatest blending of Indian and Hispanic cultures took place in the nation's center, Zavala explained, making that region the cradle of Mexican civilization. In contrast, he wrote, the northern frontier "can be considered only a source of social peculiarities."[65]

One of the sources of those peculiarities was the United States, with which Mexico has shared a shifting frontier. Contact with North Americans, which increased dramatically in the first half of the nineteenth century, began to "Americanize" Mexican culture even before the United States invaded northern Mexico in 1846.[66] Following the conquest and acquisition by the United States of the Mexican frontier in 1846–48, zones of interaction between Hispanics and Anglos continued to exist. By some definitions, these zones of intercultural contact represent frontiers, but not in the sense that Turner meant when he defined the Anglo-American frontier.[67] Hispanics seldom found opportunity in frontier California, Arizona, New Mexico, and Texas. To the contrary, Hispanics found opportunity denied them as they became victims of racial discrimination and

Interpretation," *Institute of Latin American Studies Occasional Papers*, no. 4 (Glasgow, 1971); and León-Portilla, "The Norteño Variety," 104–07.

[63] Zavala, as quoted in Weber, *Mexican Frontier*, 283. I discussed this point further. Also see Hennessy, *The Frontier in Latin American History*, 13, 14, 26.

[64] Turner, "Significance of the Frontier," 5; and Smith, *Virgin Land: The American West as Symbol and Myth* (1950; reprint edn., Cambridge, 1970), 4, 250–60. Also see Powell, *Mexico's Miguel Caldera*, 226. Powell has lamented that the American "western" of fiction and film has had no counterpart in a Mexican "northern," and he characterized the Mexican north as "an almost forgotten historical world" in Mexican popular culture. Also see Hennessy, *The Frontier in Latin American History*, 3, 13, 21.

[65] Zavala, "Frontiers of Hispanic America," 51.

[66] Weber, *Mexican Frontier*, 283.

[67] Forbes, "Frontiers in American History," 65, 68–69.

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xenophobia. Although some individuals managed to make their way up the socioeconomic ladder, Hispanics as a group were pushed to the bottom rungs by biased individuals and institutions—forced to become foreigners in what had been their native land. Hence, Turner's thesis has held no interest for historians studying Hispanic-American frontiers in the United States in the last half of the nineteenth century,[68] although this period has been especially fertile for applications of the Turner thesis to the Anglo-American westering experience, aspects of which Turnerians have named the cattlemen's frontier, the transportation frontier, the miners' frontier, the farmers' frontier, and the Indian frontier.[69] It should not be surprising, however, that there has been no corollary study of the "Hispanic frontier" following that group's subjugation at mid-century.

The Turner thesis, then, has had little direct influence on Borderlands scholarship. Although Bolton endorsed Turner's mode of analysis, neither he nor his disciples attempted to apply it. Only a few historians have explicitly tested Turner's ideas in studies of Hispanic frontiers in what is now the United States, and these scholars have not been of the Bolton school. Turner's interpretations, however, have probably influenced some Borderlands scholars who do not explicitly refer to him. His remarkable success in challenging the idea that the "germs" of European institutions planted themselves in North America and spread westward unchecked has led to a new conventional wisdom. It appears that most Borderlands scholars see no need to cite Turner's works or to carry on a running dialogue with him when they assert that the frontier altered the society and institutions of Hispanics.

If the Turner thesis has had little influence on Borderlands scholars in the past, it seems even less likely to gain their interest in the future. The thesis has been modified to the point that historians have difficulty either embracing it as an explanatory device or using it as a foil. Turner's original statement in 1892 was clear: "The existence of an area of free land, its continuous recession, and the advance of American settlement westward, explain American development." Turner and his defenders, however, subsequently refined the thesis so that it has gained sophistication but lost the elegant simplicity and force with which Turner's rhetorical excesses originally endowed it.[70]

Meanwhile, new and more satisfying explanations about frontier interactions have arisen on the foundation that Turner built, and historians have turned increasingly to them. The work of scholars such as geographer Marvin Mikesell, anthropologist Owen Lattimore, and ethnohistorian Jack Forbes have proved

[68] See, for example, Weber, *Foreigners in Their Native Land*; Leonard Pitt, *The Decline of the Californios: A Social History of the Spanish-Speaking Californians, 1846–1890* (Berkeley and Los Angeles, 1968); Arnoldo de León, *The Tejano Community, 1836–1900* (Albuquerque, N.Mex., 1982); and Robert J. Rosenbaum, *Mexicano Resistance in the Southwest: "The Sacred Right of Self-Preservation"* (Austin, Tex., 1981).

[69] These terms are taken from titles of books in the Histories of the American Frontier series, conceived and first edited by Ray Allen Billington and now edited by Howard R. Lamar, Martin Ridge, and David J. Weber and published by the University of New Mexico Press.

[70] Turner, "Significance of the Frontier," 3. For Turner's rhetoric, see Ronald H. Carpenter, *The Eloquence of Frederick Jackson Turner* (San Marino, Calif., 1983).

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especially useful, for they remind us that a frontier represents a human as well as a geographical environment. We no longer think of the frontier as a line between "civilization and savagery" but as an interaction between two different cultures. The natures of these interactive cultures—both the culture of the invader and that of the invaded—combine with the physical environment to produce a dynamic that is unique to time and place. Finally, it seems clear that Borderlands historians, like their counterparts in the history of the American West, have found other avenues of inquiry that take them beyond the Turner thesis, even if they have not yet found a substitute for it.[71]

[71] On the history of the American West, see Malone, *Historians and the American West*, 8; and Gene M. Gressley, "Whither Western American History? Speculations on a Direction," *Pacific Historical Review*, 53 (1984): 493–94. No other overarching explanatory device has replaced Turner's thesis in Borderlands history, but sociologist Emmanuel Wallerstein's world systems theory has found growing acceptance among social scientists who work in the border region, even if historians are embracing it timidly. See, for example, the special issue of *Review*, vol. 4 (1981). Also see Timothy G. Baugh, "Southern Plains Societies and Eastern Frontier Pueblo Exchange during the Protohistoric Period," *Papers of the Archaeological Society of New Mexico*, 9 (1984): 157–67; and Thomas D. Hall, "Varieties of Ethnic Persistence in the American Southwest" (Ph.D. dissertation, University of Washington, 1981). In *Mexican Frontier*, I applied Wallerstein's model to the Southwest when it belonged to Mexico.

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Reviews of Books

GENERAL

KEES VAN DER PIJL. *The Making of an Atlantic Ruling Class*. London: Verso; distributed by Schocken, New York. 1984. Pp. xviii, 331. Cloth $30.00, paper $9.50.

This book attempts to describe the transatlantic circuits of control by which North American and European capitalists sought to create a twentieth-century order that would preserve their ascendancy against revolutionary socialists and state monopolists. According to Kees van der Pijl, the driving force behind this internationalist crusade was an American corporate liberalism that fused Fordism with Wilsonian democratic universalism. Fordism combined the rationalization of production with the expansion of demand, thereby defining the basis of a class compromise in America while at the same time reinforcing the hegemony of the capitalist class. Democratic universalism extended this class compromise to the international arena.

The first one hundred pages of this book deal with the transformation of internal class structures in America (and in Europe) from the Wilson administration through the New Deal, when corporate liberalism established itself as the dominant pattern of class collaboration. The triumph of corporate liberalism in the United States then set the stage for three campaigns to re-create the American system on a global scale. These three campaigns—what van der Pijl calls the Roosevelt, Marshall, and Kennedy offensives—had qualitative differences growing out of the strength of American capital relative to that of European capital. Under the Atlantic charter universalism of FDR, American capitalists sought a global liberal system in which the United States would supplant Great Britain as the hegemonic power. During the Marshall offensive of the postwar period, this universalist idea was replaced by a more modest concept of Atlantic unity in which American capital would enjoy hegemony over European capital, would collaborate with Britain to maintain the status quo in the white-ruled Commonwealth, and would seek to sterilize the emerging Third World against Soviet-inspired insurgencies. The final offensive, Kennedy's "Atlantic partnership," sought much the same objectives but made allowances for the decline of America's relative economic position, the growing self-confidence of Europe, and the concomitant need for American-European partnerships. The Kennedy offensive had petered out by the time of the Nixon administration, largely because fundamental shifts in the American and international economies had undermined the traditional basis of corporate liberalism.

This example of Marxian class analysis offers a provocative account of how the process of class formation and collaboration affects international politics. But van der Pijl fails to achieve his main purpose—to reveal the process by which an *Atlantic* ruling class of industrial and finance capital was formed. Actually, he offers a more limited explanation of the shifting alliances among American capital and of the impact of American financial and industrial might on Europe. His attention is usually concentrated on the western shores of the Atlantic, and his implication is that the process of European class formation was almost wholly determined in response to American initiatives. This excessive concentration on American offensives is matched by a reliance on Marxist jargon that leaves the reader feeling annoyed. The prose is often indecipherable and the interpretive framework so rigid as to ignore many of the complexities and nuances brilliantly described by Charles S. Maier and other writers. Indeed, van der Pijl tends to cast events in the light of a conspiracy on the part of transatlantic capitalists against those striving for a just international order. The tone of his narration is neatly captured in the final sentence of the book, when he issues an appeal for "restoring global anti-imperialist unity" (p. 276).

MICHAEL J. HOGAN
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JOYCE GRIGSBY WILLIAMS. *Colonel House and Sir Edward Grey: A Study in Anglo-American Diplomacy.*

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Lanham, Md.: University Press of America. 1984. Pp. 174. Cloth $22.75, paper $10.75.

In her concise book Joyce Grigsby Williams provides an insightful analysis of the relationship and influence of two important protagonists in Anglo-American relations in the period from 1913 through 1919. She shows how Colonel Edward M. House, the trusted adviser and confidant of Woodrow Wilson, manipulated the president, who came to believe that House was an extension of himself. The colonel thus became "the most important executive agent in American history" (p. 21). On his first two missions to Europe, in 1913 and 1914, House cultivated a close relationship with the British foreign secretary, Sir Edward Grey. In describing House's 1913 mission to Great Britain in which House deliberately misrepresented Wilson's purpose in Mexico, Williams exposes the tenuous nature of the relationship between House and Wilson. The president placed faith in House without understanding his adviser's motives or actions, exercised little control over him, and remained unaware of how freely House disregarded his instructions. Although House's 1913 mission prolonged the misunderstanding between Wilson and Grey over Mexico, it proved useful to the British foreign secretary, who learned that Wilson was friendly toward England and that House was close to the president. Through correspondence and subsequent annual visits, Grey deliberately cultivated his relationship with the eager but naive American emissary in order to fulfill one of his wartime goals—strengthening Anglo-American relations.

In analyzing the most significant episode in this developing relationship, Williams argues that Grey's motive in creating the House-Grey memorandum of February 22, 1916, was to preserve American friendship and cooperation at a time of sharp British pressure on the American government to end neutral commerce. The foreign secretary needed to know how far Britain could go in interfering with American shipping, and the memorandum did indicate toleration of some British actions. Moreover, it served as proof to the Allies of an American bias toward their cause. Since Grey was aware, more than were House and Wilson, of the great differences between Allied war aims and Wilson's goals for peace, he delayed implementation of the provisions of the memorandum to prevent a potentially irreparable strain in the relationship between the two countries in 1916. Williams concludes that, even though House was "outmaneuvered by another professional" (p. 149), he realized that Grey's ideas matched Wilson's idealism, and he served as a catalyst between Grey and Wilson in strengthening the idea of collective security that ultimately led to the League of Nations.

This volume is important for its convincing interpretation that Grey successfully manipulated House to preserve Anglo-American friendship. Representative of recent scholarship, which stresses the importance of Grey's role in maintaining cordial relations during the period of American neutrality, Williams's book contains the best available analysis of the House-Grey memorandum. Even though Williams failed to stress the importance of Ambassador Cecil Spring Rice's reports as a source of Grey's information on Washington's policies and personalities or to use the British Foreign Office document series 371 for candid analyses of House's missions, her work makes a useful contribution to the scholarship on this period.

SHERRILL BROWN WELLS
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Department of State

A. LENTIN. *Lloyd George, Woodrow Wilson, and the Guilt of Germany: An Essay in the Pre-History of Appeasement.* Baton Rouge: Louisiana State University Press. 1984. Pp. xiii, 193. $25.00.

The thesis of this book is straightforward and familiar. A. Lentin contends that the British, standing between incompatible American and French policies at the Paris Peace Conference, were chiefly responsible for transforming Germany's seeming acceptance of "defeat" on the basis of Woodrow Wilson's Fourteen Points into Allied demands for German "surrender" on the basis of the Treaty of Versailles. But the moment the prospective Wilsonian peace became Carthaginian—symbolized by the "war guilt" of Article 231 and the reparations assessment—the British renounced their handiwork. Thus, revisionism—or "the worm of Appeasement" (p. 136)—was embedded in the treaty and underlay the victors' "crisis of will" (p. xi) in the 1930s that made them virtual handmaidens to Hitler's assault on the European order.

Lentin seeks to spread criticism fairly. Wilson erred gravely by allowing the British to count pensions in the reparations assessment and to assert Germany's war guilt, but he was of noble vision and effort and sought an honorable peace. Georges Clemenceau—predictably—sought ironclad security and restitution for France, while he also believed in the severe treaty he signed and opposed any apology. The "epoch-making" *Economic Consequences of the Peace* (1919) defamed the peacemakers and inspired the notion of a *Diktat* long before the Nazis, but John Maynard Keynes's advice was consistent, his motives pure. And the Germans had reason to resist sole blame for the war, even if their calculated rejection of the treaty and scuttling of their fleet inflamed Allied hostility.

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But most objectionable and damaging, Lentin says, was David Lloyd George's "lack of principle" (p. 120). Expediency and quest for power governed his tactics and compromises. The prime minister raised hopes for a liberal peace early in his 1918 campaign and with his Fontainebleau Memorandum and *Westminster Gazette* article in March 1919. But he always sought to appease his political hawks—particularly Lords Cunliffe, Sumner, and Northcliffe and Australia's prime minister, William Hughes—and they won his soul. Lloyd George supported their claim to a high reparations sum and then backed an indefinite figure, which raised rather than calmed public passion. Similarly, his war guilt clause proved not to be a "safety net" for conceding in Article 232 that Germany could not pay for all war damages but instead inspired Germans to seek to disprove their guilt in order to disavow reparations and much else. Lloyd George's unprincipled compromises, Lentin concludes, produced a bad treaty and the British "bad conscience" that yielded the "victory" of 1918 to the Germans at Munich in 1938.

Unfortunately, while this artfully if literarily overwritten, book makes interesting reading, the focus on war guilt and reparations is too narrow and lacks reference to recent studies of the immense political and economic significance of reparations, and the link between the policies of the disaffected liberals and radicals of 1919 and misguided appeasement in the 1930s is too facile. After all, it was chiefly British conservatives who sought to appease Hitler, to exclude the Soviet Union, and to avert ties with America's Wilsonians.

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Boston University

DOUGLAS LITTLE. *Malevolent Neutrality: The United States, Great Britain, and the Origins of the Spanish Civil War*. Ithaca: Cornell University Press. 1985. Pp. 290. $29.95.

As a title, Lord Strabolgi's phrase "malevolent neutrality" accurately conveys the frustration and anger of many on both sides of the Atlantic at the adoption of policies denying aid to the beleaguered Spanish Republican government at the outbreak of the Civil War in July 1936. After introductory chapters surveying Spain's position following World War I, Douglas Little's book sets out to show, first, how the unrest of the first five years of the Second Spanish Republic stimulated fears of communism in both American and British government circles and, second, how these fears hardened into the anti-Republican policies of nonintervention in Britain and a "moral" and, later, formal embargo on arms exports from the United States. Neither of these propositions is new, but the interest of the book lies in the use the author has made of the relatively underworked archival material from the State Department and the Foreign Office files for the years 1931–35, on which the central third of his book is based. The remaining third deals with the introduction and implementation of these policies, and here much of the documentation presented will be familiar to scholars of Spanish Civil War history from secondary works.

One difficulty for the historian of the early 1930s is that, since Spain's role in world politics before the Civil War was a fairly minor one, the diplomatic material available is inevitably limited. Even so, the author has largely passed by some questions raised in these papers and bearing on official opinion—such as Spain's position vis-à-vis the League of Nations, Latin America, and French Morocco—to deal almost exclusively with the vagaries of trade and commerce universally experienced at this time. Here some help from tables would have assisted the illustration of what are detailed and complex matters.

What emerges from Little's examination of the diplomacy of the early years of the Second Republic is a marked difference in response between the State Department and the Foreign Office to events in Spain. The fate of trade and investment in Spain attracted considerable attention at a high level of the State Department, and even on occasion from Franklin Roosevelt himself. In contrast, the Foreign Office took a fairly sanguine view of events, with reports from Spain seldom going beyond the League and Western Department, which dealt with Spain among its other pressing concerns. The author, however, fails to make clear that incoming telegrams did not necessarily represent Foreign Office opinion and that dispatches formally addressed to the secretary of state were not all read by him. Moreover, his synecdochic use of "Whitehall" for the Foreign Office or even junior secretaries is misleading, and sober assessment is further fudged by a tendency to inflate. Was "Washington" really "stunned" (p. 115) or "Whitehall" "shocked and angered" (p. 219) by setbacks in economic negotiations at a time when the Far East was in turmoil and the future of the British empire and even of Europe was in question?

Other studies have pointed to both instability in Spain and "anti-bolshevik" sentiment as primary factors in the formulation of policies in the United States and in Britain. But there is little evidence that Stanley Baldwin was more influenced by the Foreign Office (like the State Department, at best equivocal on Spain) than by the views of acquaintances, the press, or even parliamentary opinion. Nevertheless, Little's book indicates some historiographical lacunae in a period where many questions on

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Spain's fairly restricted international role remain to be answered.

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ALAN WATSON. *The Evolution of Law*. Baltimore: Johns Hopkins University Press. 1985. Pp. xi, 156.

In the field of Roman and comparative law, Alan Watson occupies a prominent position. He has written more than a dozen major books on legal subjects in general and in the history of the later Roman Republic. The book under review deals with general themes in Roman and comparative law to support the radical hypothesis "that legal development is to some considerable extent distinct from other developments in the society and in large measure is dictated by the tradition, relating to the law, that prevails among the legal elite" (p. 93). Watson contends "that law is largely autonomous and not shaped by societal needs . . . [yet] evolves from the legal tradition . . . [and] that for satisfactory law in society one must have a satisfactory legal tradition . . . [for] law is typically backward-looking . . . [and] inherently conservative" (p. 119). As the title of chapter 4 ("Evolution and Revolution") suggests, Watson believes "that an examination of revolution in law confirms the general thesis of the enormous impact of the legal tradition on legal change" (p. 110).

The author's one-sided emphasis on tradition and evolution at the expense of innovation, change, and revolution contrasts with Harold J. Berman's approach in his recent *Law and Revolution: The Formation of the Western Legal Tradition* (1983). Berman stresses the importance of revolutionary change for the development of legal tradition, as viewed against the formative but not determining influence of societal factors. No doubt, the ultimate solution to this important debate will be to combine both extremes, but neither of these two authors makes use of the other.

The main categories that Watson uses to substantiate his thesis are the Roman system of contracts (chap. 1), customary law (chap. 2), the cause of the reception of Roman law (chap. 3), and evolution versus revolution (chap. 4). One could choose other categories and obtain different results. Watson conspicuously neglects, for example, the making of legislation, in which creativity, coercion, change, society, and innovation inevitably play crucial roles. He makes sporadic, often unclear, references to the role of "lawmakers," defined broadly as "those lawyers who, whether as legislators, jurists, or judges, have control of the accepted mechanisms of legal change" (p. 118). Watson did devote a separate book to lawmakers in the later Roman Republic, but there he focused on the formation of private, rather than public, law by state assemblies, magistrates, and jurists, concluding that magisterial edicts were the most important source for the development of law; such edicts were obviously more constrained by tradition than the law laid down by later Roman emperors or Renaissance kings. In that book as well, Watson neglected legislation as a coercive act of will and change by an authoritative single ruler. His specialization in the relatively limited views on the lawmaking power in the Roman Republic has perhaps conditioned his treatments of the general principles of lawmaking.

This is a bold, brilliant, and provocative book that will occasion much debate in legal circles. It is one of a growing number of books on European law that challenge the deterministic views of some social historians and that show that law is at least as important a factor as, for example, economics. Although Watson has treated similar subjects elsewhere, he is here more radical, and it is to be hoped that he will develop his present hypothesis beyond the short and condensed confines of this book. His chapter bearing the tantalizing title "The Cause of the Reception of Roman Law" promises more than he can deliver here, but it is nonetheless a most valuable contribution.

A. LONDON FELL
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MARTIN VAN CREVELD. *Command in War*. Cambridge: Harvard University Press. 1985. Pp. vii, 339. $20.00.

Martin Van Creveld intends to concentrate on command, control, and communications in this book, which contains eight chapters: "Introduction: On Command," "The Stone Age of Command," "The Revolution in Strategy," "Railroads, Rifles, and Wires," "The Timetable War," "Masters of Mobile Warfare," "The Helicopter and the Computer," and "Conclusion: Reflections on Command." It is not a study of strategy and tactics. If anything, the author emphasizes control and communications more than questions of command. Van Creveld tries to write these "case studies . . . as far as possible, through commanders' eyes and from behind their desks" (p. 15). The primary audience for the book appears to be military officers, to judge from a prefatory remark: "Even without firm conclusions, the discussion itself may be valuable. . . . Military history may be an inadequate tool for commanders to rely on, but a better one has yet to be designed" (p. 15). The chronological span is from classical antiquity to the present, and the author adopts a self-consciously contrary approach.

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Van Creveld is weakest in discussing antiquity, which he does not know well. There are, however, other problems with his book. It shows some signs of haste in preparation, it is much too diffuse, and the analysis jumps too abruptly and unevenly. In several case studies his analysis becomes unclear while failing to preserve the interconnections and development that the author wishes to trace. His most detailed historical analysis concerns two nineteenth-century generals, Napoleon and von Moltke, and their problems of command. Surprisingly, World War II receives cursory attention. The transition from World War I to a well-informed discussion of problems and capabilities of command within the Israel Defense Forces in the post-1945 era is too abrupt. This book is insufficiently rigorous for most historians. It involves no archival research and is derivative without being really synthetic. Van Creveld is necessarily dependent on his sources.

An analysis of problems of command in historical perspective is, in principle, worthwhile. *Command in War* may have value in some college and university courses on military history, for it is a thought-provoking book. Van Creveld's observations are often astute and his style can be witty. His conclusions do contain some reflections that may be helpful. This study can also alert the reader to problems and can cause the reader to think about certain otherwise-neglected dimensions of military history. It is refreshing to have a scholar write about military aspects of military history; Van Creveld need not apologize for such a concentration (p. 13). Some of the endnotes are helpful, and the bibliography can serve as a useful introduction to many topics. But the book as a whole is disappointing. Van Creveld has written better ones. Whether this one will have practical utility for commanders is another and still open question.

WALTER EMIL KAEGI, JR.
University of Chicago

WALTER MINCHINTON *et al*, editors. *Virginia Slave-Trade Statistics, 1698–1775*. Richmond: Virginia State Library. 1984. Pp. xvi, 218.

JEAN METTAS. *Répertoire des expéditions négrières françaises au XVIII^e siècle*. Volume 2, *Ports autres que Nantes*. Edited by SERGE DAGET and MICHÈLE DAGET. (Bibliothèque d'Histoire d'Outre-Mer, new series, Instruments de Travail, number 2.) Paris: Société Française d'Histoire d'Outre-Mer; distributed by L'Harmattan. 1984. Pp. ix, 972. 750 fr.

For several decades, Elizabeth Donnan's *Documents Illustrative of the History of the Slave Trade to America* (1930–35) was the single outstanding contribution to slave-trade studies. Then, from the late 1960s onward, historians turned to the closer analysis of the sources that were already available. Now, in the 1980s, we seem to be experiencing a return to the publication of documents in the Donnan tradition.

Although the two works under review are both essentially annotated lists of slave ships, they are quite different in source and intent. The Virginia volume records arrivals of slavers at a slave-trade destination, whereas the French volume covers whole voyages from a French port, to Africa, then America, and back to France.

The Virginia records are those kept by the naval officers in various Virginia ports. Naval officers in this case had nothing to do with the Admiralty. Instead, they were the officials charged with keeping track of the movement of shipping as part of the apparatus of imperial control over trade and navigation. The result was a ship-by-ship listing, giving date of entry, master's name, tonnage, number of guns, number of crew, place of construction and registration, owners, port of departure, port of entry in Virginia, and the place and date on which bond was given for the voyage. These lists were compiled in the colony and sent each quarter to London, where they have now been reassembled from the files of the Public Record Office.

Donnan published some of these records in her earlier compilation, but this list is far more complete. It does, however, preserve Donnan's original annotation, and the present editors have done a meticulous job of further annotating the list from contemporaneous newspapers and other sources. Separate indexes cover all mention of owners, masters, and individual ships. As might be expected of returns from overseas, gaps sometimes occur in the records, but the sources are complete enough to give a comprehensive picture of the slave trade to Virginia between 1698 and 1775, and they are especially valuable for showing the organization and finance of the trade.

Useful as the Virginia volume promises to be, Jean Mettas's volume is richer still. Before his untimely death, Mettas was at work on a general survey of the French slave trade of the eighteenth century. He left individual note slips for all the voyages he could find in the French records. Serge Daget and Michèle Daget then prepared these records for publication—in two volumes, the first, which appeared in 1978, covered the 1,427 recorded sailings from the port of Nantes; the present volume completes the collection with information on voyages from all other French ports—1,916 individual voyages from twenty-one different ports. The extent of Mettas's research is staggering. Aside from the various offices of the French national archives in Paris, he consulted the municipal or departmental archives in nineteen French cities and archives in Portugal and England as well.

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The information provided about each voyage is somewhat more complete than that of the British naval officers, partly because the French officials recorded information about complete voyages from home port to home port. The Dagets' systematic database includes tonnage, beginning crew number, crew deaths, names of the captain and his replacement (if any), the names of the owners, date of outfitting, date of departure, and names and dates of ports of call in Africa. The lists include the numbers of slaves loaded (separating men, women, and children in many cases) along with the number of deaths and, when available, the time of their occurrence. For the American side of the trade they include the ports of call and the sales of slaves in each, followed by the date of return and decommission of the vessel and a narrative description of any particularities of that voyage. And the whole is completed by Jean-Claude Nardin's excellent indexes—one each for names of ships, owners, and captains, ports of call in Africa and in the Americas, and ports of return in Europe. He also provides an excellent analytical index of materials and events reported.

It is clear that, with such a wealth of conveniently arranged information, both volumes are invaluable works of reference and will no doubt serve as a base for many studies of the economic history of the Atlantic world in the eighteenth century.

PHILIP D. CURTIN
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MICHAEL ANN HOLLY. *Panofsky and the Foundations of Art History*. Ithaca: Cornell University Press. 1984. Pp. 267. $24.95.

In his book *The Critical Historians of Art* (1982) Michael Podro provided us with a fairly comprehensive and very incisive exposition of the great tradition of German art history. Michael Ann Holly's new work is a welcome complement to this, concentrating as it does on one of that tradition's most profound and influential figures.

The introductory part of Holly's book outlines those crucial theories and debates about historical and art historical understanding that were current during Erwin Panofsky's formative years. She then considers the relation between Heinrich Wölfflin and Panofsky, with particular reference to the latter's essay "Das Problem der Form in der Bildenen Kunst" (1915). Although Holly finds this piece "frustrating in its vagueness about the connection between mind, culture, and content in art" (p. 67), she nevertheless sees it as an important sign of Panofsky's developing interest in the artwork as "a historically revealing intellectual document" (p. 68). This interest is equally manifest in Holly's discussion of Alois Riegl and Panofsky. Again, she concentrates on a specific paper, this time Panofsky's "Der Begriff des Kunstwollens" (1920). In this piece he is shown to have been moving beyond Riegl's empiricist psychologistic interpretation of the *Kunstwollen* toward a more Kantian, epistemological orientation. This neo-Kantian dimension of Panofsky's thought is (after a brief survey of other relevant debates in early twentieth-century art history) given a much more detailed consideration in Holly's long section on his relation to the philosophy of Ernst Cassirer. Here "Die Perspecktive als 'symbolische Form'" (1924–25) is the focus of discussion. In particular, Holly is anxious to show that, although Panofsky affirms the conventional foundations of perspective, he regards it (in Holly's words) as "expressive of a creative interdependence of the mind and the world" (p. 156).

The final section is devoted to Panofsky's mature theory of iconology. Having outlined his three levels of approach to art historical understanding—"preiconographic description," "iconographic analysis," and "icononological interpretation"—Holly concentrates on the meaning and significance of the third of these. This enables her both to defend Panofsky from those who find him insensitive to art as art and to make some broader links, which suggest that his notion of iconology anticipated recent important trends in, among other fields, semiotics.

The great merit of Holly's book is that it not only provides a much-needed introduction to Panofsky's thought but also does so in a way that convincingly, yet critically, illuminates the internal continuity of that thought. It is especially valuable to have such detailed analyses of the three early papers, two of which have not yet been translated into English. At least one problem, however, must be noted. Holly raises matters of general theoretical and methodological significance throughout the book, especially in the final section. Her treatment of these, nevertheless, remains somewhat incomplete. In particular, the question of criteria for determining the validity of iconological interpretations is not given adequate consideration. Without such criteria, the way is clear for an empiricist skepticism toward the worth of iconology as such.

PAUL CROWTHER
University of St. Andrews

ANCIENT

LAWRENCE KEPPIE. *The Making of the Roman Army: From Republic to Empire*. Totowa, N.J.: Barnes and Noble. 1984. Pp. 271. $26.50.

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Not a book on the army of the Republic? Can there be *anything* new to say on the five hundred years down to Caesar when, for lack of archaeological and documentary sources, we have to turn for information to the long-exhausted mines of Polybius and Livy? To be sure, for the five hundred years of the Empire, new and exciting finds are made every day, as buildings, roads, weapons, fortifications, writing tablets, inscriptions, papyri, coins, and works of art are unearthed, adding luster and sparkle to an already rich and intriguing picture. Carrying "the story of Rome's army from a militia guarding a village on the Tiber down to the mid-first century AD," the book draws its life from the use of this later wealth. It is well put together, precisely because it is mercifully short on the earlier period for which our sources are so spotty and because it goes on into the well-documented imperial period to an otherwise unjustifiably late cutoff date.

The book is written for the general reader, but fellow researcher finds some interesting pointers by hunting through the appendixes and notes and is well served by the fine bibliography. Much of the best has been written in recent years and is found in this wise, though somewhat insular, selection (Keppie makes no reference to the studies of such non-British scholars as J. F. Gilliam or K. Kraft).

The plates in figure 5 show four soldiers putting their index fingers on their sword hilts in a well-known ceremonial salute in the Roman army. Two coins (fig. 17 c and d), showing Caligula and Nero addressing troops, read *adlocut(io) coh(ortium)*, which surely means the praetorian cohorts, not the men of four legions or the German bodyguard. The latter was not a squad (p. 154) but a regiment of 500 or 1000 men. The old attention-getter that Roman troops, captured by the Parthians, may have fought the Chinese (p. 237) has long been laid to rest and should not have been permitted again to stalk the pages of a scholarly work. By contrast, the author's own research on sling bullets of the Perusine War, with the exciting hint of newly found, inscribed bullets from the Hannibalic War in Spain, and on veteran settlement in Italy are highlights of this welcome, balanced volume.

M. P. SPEIDEL
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K. R. BRADLEY. *Slaves and Masters in the Roman Empire: A Study in Social Control.* (Collection Latomus, number 185.) Brussels: Latomus. 1984. Pp. 164. 750 F.

In recent years studies of slavery have proliferated in many fields, including ancient Rome. K. R. Bradley, consequently, is able to draw on a rich legacy of secondary works as well as on the copious legal, literary, and inscriptional sources to present an informed interpretive essay on the means of social control inherent in the system of slavery in imperial Rome. Bradley argues that Roman slavery was harsh and even its supposedly benign features were in fact props for the system. His interpretation rests on a series of highly plausible hypotheses about the attitudes and actions of slave owners and slaves alike. His concern to use the sources produced almost wholly by the elite to gain a picture of the mentality of the slaves is especially praiseworthy.

The Roman slave owners maintained a system of governing their slaves through reward and punishment, and they got their slaves to participate in their own enslavement, that is, to strive for the rewards and to avoid the punishments. Even the incentives and rewards, which could be withheld or revoked at the owner's whim, were double-edged. For example, although marriage between slaves was illegal, slaves were able to form families, with at least the tacit permission of their masters. Nevertheless, the distinct possibility existed that slave families would be disrupted by being sold. The slaves' best hope of maintaining their hard-won privilege was to exhibit compliant attitudes, precisely the ones the slave owners hoped to instill in their slaves.

Masters also used manumission as an incentive to secure the compliant behavior of their slaves. Manumission was always the master's gift, and by no means all slaves could hope to attain it, even if the master agreed. A magistrate had to record the deed, and in some areas magistrates were not always present. Where they were, they were not always willing to divert their attention and deal with all cases of manumission. The fee usually charged for manumission was both an incentive for the slave and insurance that the master got better work from his slave as well as a cash payment and continuing obligations from his freedman. In discussing the Augustan legislation on manumission, Bradley shows that the three laws (the third of which may be from the reign of Tiberias) were intended not to stop manumissions nor to bar Greeks and Orientals from citizenship but to define the criteria for full citizenship and to assert a kind of quality control over those who received it.

When the less harsh and quasi-benign methods of manipulation failed, Roman slave owners had recourse to violence. Slaves could find themselves personally humiliated, sexually abused, or physically punished; the laws that limited such acts by the master were partial and hard to enforce.

Much is familiar in the individual components of Bradley's line of argument and in the myriad bits of evidence he cites to support it. The value of the book comes from the coherence and persuasiveness of his interpretation. Many of Bradley's hypotheses cannot be proved, but they seem correct in light of

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what we know about the Roman and other slave systems.

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San Diego State University

MEDIEVAL

THOMAS F. X. NOBLE. *The Republic of St. Peter: The Birth of the Papal State, 680–825.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1984. Pp. xxix, 374. $35.00.

This study deals with an important subject that has largely been the preserve of German and French historians. For this reason alone, the book will be welcome to English-speaking medievalists and their graduate students. Sifting painstakingly through a century of German, French, and Italian historiography, with emphasis on English contributions when available, Thomas F. X. Noble relies primarily on the papal lives of the *Liber pontificalis* and the collection of correspondence known as *Codex Carolinus* to argue that the papal state (a term he rejects in favor of "Republic of St. Peter" or simply "Republic") was consciously created by consistent papal policies. Noble employs Sidney J. Ehler's basic definition of a state, which requires the state to have "a territory, a population and an established governmental power" (p. 325). He seeks to prove that, given certain modifications of Ehler's definition, the republic of St. Peter was a full-fledged state prior to 800 and, afterward, an autonomous region in the Frankish empire, so autonomous indeed that it was practically independent. The emphasis throughout the book lies on papal leadership for Italian aspirations, and the author thus challenges the Byzantinist and Frankish theses of the origin of the papal state. He does not deny "that developments in Byzantium played a key role in precipitating . . . crises in Italy" (p. xxiii) and is willing to accord the Franks "an auxiliary role" (p. xxv), but he intends to "reclaim for the Republic its own history," which he locates "squarely in Italy" (p. xxv).

The task is not an easy one. In the major part of the book, Noble examines the relationship of the papacy with Franks and Lombards. The following pages describe the features, functions, and governance of what Noble describes as a new political entity, the republic. Since relevant primary sources are scanty, contradictory, and subjective (they are unfortunately never specifically discussed in the book), they have often been the object of more or less ingenious explanations. This is particularly true for the Carolingian donations and promises. In 1893 Paul Kehr remarked that any discussion of the "Roman question," though essential in the context of Italian territorial development, is at the same time a very daring and an almost hopeless undertaking given the innumerable earlier attempts by the most outstanding scholars of their time. Undaunted, the author pursues the various strands of argumentation and presents a synthesis of earlier thought and facts to support his own arguments.

Inevitably, the unwary reader may sometimes be misled, especially when references in the footnotes are quoted out of their original context. An example is Noble's and Theodor Schieffer's evaluations of Pope Zachary, the last Greek on the papal throne. In *Winfried-Bonifatius* Schieffer sees Zachary as a deeply loyal subject of the Byzantine empire, who was obliged by contemporary events to negotiate with the Lombards. By contrast, Noble sees Zachary as an independent sovereign by intent and, thus, an opponent of the basileus, who took advantage of the contemporary situation to further papal interests, which Noble equates with Italian national aspirations. In rejecting Greek influence on the papacy and Rome in general, Noble draws on J. Richards's *The Popes and the Papacy* (1979). But Richards sees the Greek presence in Italy as a significant development, whereas Noble allows for the seventh century at most "a residual feeling that Hellenization constituted a potential danger to the purity of the Roman tradition" (p. 278).

Another example is Noble's reference to an article, by Peter Classen, that influenced him particularly. Noble argues that part of Charlemagne's imperial title, "Romanum gubernans imperium," was "a formula . . . designed to include the Romans in the empire without acknowledging that the empire itself was Roman. . . . Charlemagne did not see himself as a Roman emperor" (p. 296). Classen's point, however, was that the phrase had been carefully selected to express unambiguously Charlemagne's "Anspruch auf das Römische Reich." Given that almost everything regarding the "republic" is controversial, Noble could not possibly have entered into more detailed historiographical discussions, but the reader must be warned that secondary works mentioned in the footnotes frequently present a picture of particular events that differs radically from the views of Noble, who courageously imposes his own interpretation on the sources.

The author is to be congratulated for a well-written, stimulating book that presents a fresh point of view with an impressive array of secondary references. It will certainly become a basic tool for discussions of early medieval Carolingian and papal history. The handsome volume contains five maps, a full bibliography, and a helpful and accurate index. Unfortunately, a number of printers' errors were overlooked.

UTA-RENATE BLUMENTHAL
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INGRID HEIDRICH. *Ravenna unter Erzbischof Wibert, 1073–1100: Untersuchungen zur Stellung des Erzbischofs und Gegenpapstes Clemens III. in seiner Metropole.* (Vorträge und Forschungen, number 32.) Sigmaringen: Jan Thorbecke. 1984. Pp. 220. DM 62.

When sources are scant, the most unpromising material can yield much if one takes the trouble. Ingrid Heidrich uses neglected charter evidence to view Clement III, the first antipope of the investiture struggle, in the context of his career as archbishop of Ravenna (1073–1100).

Although Guibert (Wibert) has been studied often and well as a supporter of Henry IV against Gregory VII and Urban II, only local historians have made much use of the eighty-two extant documents issued by his archiepiscopal chancery, among which leases either for life (*emphyteusis*) or for twenty-nine years (*libellus*) predominate. Diplomatically they are remarkable for their traditional form, which contrasts with that used by Guibert as pope (unlike other antipopes, he maintained separate chanceries for his two offices).

Although Ravenna's suffragans extended along the Via Emilia all the way to Parma, the church's property holdings in this direction stopped at Bologna; in other directions, Ferrara was the northern limit and Senigallia the southern. Apparently, Ravenna's earlier holdings beyond these limits, which the Lombard king Liutprand had conquered between 727 and 744, were not restored by the Franks. Continuity with the old Byzantine exarchate and Pentapolis is also seen in the titles of *comes* and *dux*, which almost always are hereditary honorifics rather than indications of office; vassalage is rarely mentioned.

Archbishop Guibert was the greatest lord in the region—his influence stopped where Matilda's began—but his city was already overshadowed economically by Venice (the charters specify payment in Venetian coins), and, furthermore, its preeminence in imperial Italy was being threatened by the rise of Milan. These pressures, together with a centuries-old rivalry with Rome over the Pentapolis, may have encouraged Guibert to be antipope. A moderate reformer himself, he opposed simony and clerical incontinence as much as any Gregorian but could not countenance the novel political means Gregory employed to achieve his ends. It now seems extremely doubtful that the forged investiture privileges commonly attributed to Guibert and his circle at Ravenna actually originated in that context; indeed, Roman law, with which the forger and other imperial publicists were familiar, does not seem to have been much studied there (Peter Damian learned his elsewhere).

These are but a few of the highpoints of a book dense with detail, presented clearly, concisely, and judiciously, yet without that tunnel vision so often found in monographs.

RICHARD KAY
University of Kansas

KENNETH M. SETTON. *The Papacy and the Levant, 1204–1571.* Volume 3, *The Sixteenth Century to the Reign of Julius III*; volume 4, *The Sixteenth Century from Julius III to Pius V.* (Memoirs of the American Philosophical Society, numbers 161 and 162.) Philadelphia: The Society. Pp. x, 564; viii, 565–1179. $90.00.

These are the final two volumes of Kenneth M. Setton's monumental work on the papacy and the Levant. Volumes 3 and 4, covering the period 1503–71, serve to reaffirm that historians are justified to include the sixteenth century in the early modern period. The analogy of seeing the known world split into two antagonistic camps—then, the Christian West professing utmost concern (though not always monolithically) with containing the advance of the Muslim Turkish East—is only too familiar in the 1980s, when the world is still seen in terms of an East-West struggle. Because the hostility now involves nothing less than humanity's survival, Setton's documentary text, though strictly factual, reads to us like a fairy tale. Considering that the pope and other European sovereigns had no greater reason for alarm than, for instance, receiving news of the increase of Turkish naval power to three hundred sail, even that war-torn, Inquisition-ridden century seems preferable to our own.

But from the perspective of sixteenth-century Europe the Turkish danger to Christendom was real enough. Between 1503 and 1532 Selim I (the Grim) had taken Egypt and Syria; his son Suleiman the Magnificent conquered Belgrade and the island of Rhodes, penetrated into Hungary, took Buda, and was stopped only before the walls of Vienna. Yet the Levant was not the chief preoccupation of the eleven popes considered here. Inter-Christian rivalry among vain and power-hungry rulers usually prevented a unified stand against the foremost enemy of the church. Although each pope was expected to call for, and organize, a crusade, this task could seldom be attempted.

During the seven decades of this story only two crusades were organized. Three popes died soon after being elected; most of the others were entangled in almost continuous wars between France and Spain (abetted by princely Italian lords and republican city-states) on Italian soil. The first to give the Turkish peril more than the "routine advocacy of a crusade" (vol. 3, p. 38) was the aged Farnese,

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Paul III (1534–49), who, with Charles V and Venice, formed a holy league against Suleiman in 1538. The enterprise, however, ended in failure, letting both pope and emperor return to their own conflicting political and dynastic interests. One of the later popes, Paul IV (Carafa, "the impossible pope" [1555–59]), attempted to enlist Suleiman's help to drive the Spanish out of Italy. But after the Treaty of Cateau-Cambrésis (1559), with Italy pacified under Spanish domination, conditions became more propitious for another crusade. Pius V's alliance with Venice and Philip II's Spain brought victory over the Turkish fleet at Lepanto (1571).

One of the few historical events celebrated by the greatest geniuses of the time in arts and literature, Lepanto seems a fitting climax to the many variations of the leitmotif of Turkish danger. But does this impression correspond to reality? Although Setton's story ends here, the East-West confrontation did not. His last chapter sums up the aftermath: the Turks soon regained their predominance in North Africa, from Tunis to Morocco; they reestablished, for the time being, good relations with Venice but arrived at peace with Spain only near the end of the century. Thereafter, the East-West struggle resumed, directed mostly against Venice and involving several seventeenth-century popes. Another holy league was formed as late as 1684 by Innocent XI, Venice, Austria, and Poland. The Muslim East, it is true, did not advance further into the realm of Spanish Christendom, but the coexistence of the slowly declining Ottoman and Spanish empires was offset by the Turks' continuing warfare throughout the eighteenth century with Venice, Austria, and other Christian peoples of Eastern Europe.

Why, then, did the author require two huge volumes to deal with a relatively short period of a continuing problem? The answer seems to be Setton's infatuation with the raw stuff of history, the immense attraction of genuine documents (much more plentiful for this period than for the medieval past), and the joy of finding out the truth from the correspondence—revealing the duplicity and even treachery with which politics was conducted—of many a crowned head and their ministers. Setton, with an enthusiast's urge to tell all, gives a solid segment of *wie es eigentlich gewesen*; this includes, within the chronological limits he set himself, the papacy, complete with its eleven conclaves, various church councils, and most of the rest of Europe from Poland and Hungary to Portugal.

His story delights with its sense of actuality as seen through the eyes of diplomats writing from capital cities or from scenes of war on land and sea. Actions involving the Turks form highlights, such as ambassadors' audiences with sultans and pashas or the siege and fall of Rhodes (1522), Chios (1566), and Famagusta (1571). The author offers innumerable new details gathered in a lifetime of research, much of it in the secret archives of Venice and the Vatican. This history, even if familiar to scholars in its main outlines, appears from Setton's angle as a new panorama. His study has no equal as an up-to-date work of reference; it excels in precision of all data, with nary a misprint, not even in the countless multilingual quotations and book titles. This magnum opus is in all respects a superlative achievement.

ERIKA SPIVAKOVSKY
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ELIYAHU ASHTOR. *Levant Trade in the Later Middle Ages*. Princeton: Princeton University Press. 1983. Pp. xxii, 599. $60.00.

The commercial relations between Europe and the Near East during the Middle Ages has been a lively theme in the historiography of medieval Christendom and medieval Islam. Henri Pirenne's seminal thesis attributed the real beginnings of medieval European society to the rise of Islam and the consequent rupture of commercial exchanges between the seventh and ninth centuries. During the fifty years since he first propounded his thesis in *Mohammed and Charlemagne*, Pirenne's conclusions have displayed a remarkable and persistent vitality. No subsequent discussion of medieval Europe's early beginnings could ignore the crucial issue of trade between East and West. Although this trade was a somewhat less prominent historiographic problem for the later Middle Ages, the evolution of this commerce was nevertheless assigned a heavy historical burden. The waning of Southern Europe and the purported general decline of Egypt and the Levant at the end of the Middle Ages were in part attributed to the severe contraction of trans-Mediterranean trade following the discovery of direct sea routes to the Indies.

Given the centrality of this question for the history of both Europe and the Middle East, it is indeed surprising that, until the publication of the work under review, the only comprehensive treatment of the trade was W. Heyd's *Histoire du commerce du Levant au Moyen Age* (1885). In this, his last and most important work, Eliyahu Ashtor does much more than update Heyd's work with the prolific results of a century's worth of relevant scholarship. Ashtor has written a completely new history of the Levant trade from 1291 to 1498 based primarily on his own research in the major European archives of the Mediterranean basin. The amount of documentary material he examined and analyzed is truly awesome, and the quantitative and other data on the

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volume of trade, prices, and commodities are entirely new.

By the end of the thirteenth century the commercial revolution of the eleventh century had already transformed Mediterranean trade relations into a regular and fairly complex pattern. In exchange for its spices, sugar, cotton, and flax the Levant imported finished textiles, furs, timber, and iron from Europe. Even the young Turkish male slaves needed to replenish the Mamluk ranks in Egypt and Syria were in a sense a "European product" since they were acquired, transported, and sold on Levantine markets by European traders. Thus, by 1291 the triangular trade between Europe, Byzantium, and the Levant was a thriving pursuit largely in European hands.

The first chapter, "The Crisis of Levant Trade (1291–1344)," traces the diminution of direct European trade with Syria and Egypt consequent on the fall of Acre—the last Crusade outpost in the Levant—to the Mamluks in 1291 and of the papal prohibition and subsequent embargo on trade with Muslims between 1291 and 1344. Levantine trade, of course, did not come to a standstill during this period. It was reduced in volume, but, more important, it was redirected toward the north following the land route from the northern coast of the Black Sea across Asia.

Ashtor skillfully structures the wealth of archival data that he uncovered into the argument of all chapters. So, for example, he offers a detailed account of how the dip in volume of Levant trade affected all its major participants. Wherever possible, his discussion is buttressed by tables summarizing the quantitative data on voyages, cargoes, and prices. Virtually all of these data are new, and all of them derive from Italian, Spanish, and French archives, which Ashtor was the first to study systematically.

In subsequent chapters Ashtor proceeds chronologically, treating the renewal of the trans-Mediterranean trade between 1345 and 1370; the significant expansion of this trade from 1370 to 1402 as a result of disruption along competing trade routes; the achievement by Venice of preeminence, but not exclusive dominance, in the Levantine commerce between 1402 and 1421; and the crisis between 1422 and 1452 resulting from increasingly harmful intervention and monopolization of the spice trade by the Mamluk state of Egypt.

In Chapter 4, "The Levant Trader at Home and Overseas," Ashtor interrupts the chronological sequence and detours into the domain of qualitative economic history. He discusses in fascinating detail the organization of the European component of the Levant trade. Large-scale merchants with an annual volume of trade of ten thousand ducats or more predominated, even though many smaller entrepreneurs continued to be active. Improved methods of business organization, bookkeeping, circulation of commercial information, and advances in shipping techniques all contributed to supporting the expansion of Levantine trade, which by 1400 had reached a volume of about 1 million ducats annually with average profits ranging from 15 percent in metals to 25 percent in spices.

Ashtor concludes his study with a chapter on the period (1453–98) during which Venice moved from preeminence to dominance. It came to control 60 percent to 70 percent of the Levant trade, while the share of other European trading partners such as Genoa and Catalonia declined correspondingly. Levantine commerce reached its apogee in the last half of the fifteenth century when it achieved an annual figure of over 1.1 million ducats a year. The advent of Ottoman rule in Syria and Egypt in the early sixteenth century did not substantially affect either the volume or the regularity of the Levant trade.

This is a history of Europe's trade with the Levant and Egypt. Although the Near Eastern side of this commerce is not neglected, the absence of archival sources comparable to those for the European side did not permit Ashtor to devote the same detailed, comprehensive attention to the Oriental components. It is a work of discovery and of synthesis, offering interpretations and generalizations on a number of levels. On the level of straightforward commercial history, his conclusions are virtually unassailable, buttressed as they are by a plethora of archival evidence. On the level of broader historical interpretation, some of Ashtor's conclusions are subject to discussion and even disagreement. For example, he argues that the commercial expansion of the European powers is directly connected to the industrial decay of the Near East. Although this may have played some role, the facts of demographic and social history were certainly of much greater importance.

Ashtor's six-hundred-page study is a prodigious work of scholarship. It will be the basis for any discussion of the subject for decades to come and is a fitting and impressive memorial to Ashtor's scholarly career.

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Princeton University

ELIYAHU ASHTOR. *The Jews of Moslem Spain*. Volume 3. Translated by AARON KLEIN and JENNY MACHLOWITZ KLEIN. Philadelphia: Jewish Publication Society of America. 1984. Pp. 310. $19.95.

In this third and last volume of his monumental study of Jews in Islamic Spain to the end of the eleventh century, Eliyahu Ashtor provides two dis-

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tinct perspectives on what he termed the symbiotic relationship between Jews and Muslims. The first of three chapters deals with the Golden Age of Hebrew literature, and Ashtor points out the influence of Arabic traditions and styles on fields as distinct as grammar, poetry, philosophy, and natural science. The discussion of grammar (in the person of the great grammarian, Abu'l-Walid) makes clear the way in which the methods and systems of Arabic grammarians provided a model for the interpretation of Hebrew language structure. The rest of the chapter is limited to two figures only, the philosopher-poets ibn Gabirol and ibn Paquda. The discussion of Hebrew poetry seems oddly dated, and the failure to treat *muwashshahs* (Hebrew poems with rhymed couplets in Romance or Arabic), a distinctive genre that has aroused much scholarly debate since S. M. Stern introduced the topic in 1948, is all the more glaring in view of Ashtor's insistence on the importance of Judeo-Arabic and Arabic/Romance bilingualism among Andalusi Jews.

The second chapter, "Diary of a Jew in Eleventh-Century Spain," is a marvelous reconstruction of daily life and customs, somewhat in the style of Israel Abrahams's *Jewish Life in the Middle Ages* (1896). Ashtor includes excellent discussions of how the communal administrative system worked, of Jewish education and the cultural and linguistic assimilation of Jews to Arabic norms, of religious life and leisure activities, based mainly on the rich vein of material contained in rabbinical responsas. That Jews picked up Islamic religious trappings such as ritual ablution and prostration during prayer is particularly revealing.

The final chapter treats the political turmoil in the party kingdoms and the role of Jews in the complex politics of the middle decades of the eleventh century. At the chapter's end the author reveals what is doubtless a "hidden agenda" in distinguishing between the courtier intellectual class—rationalist in philosophy, relativist in religion—and a more orthodox group "close to the simple folk" who preserved religious and ethnic traditions. The courtier class, he states, "regarded themselves as real Spaniards and had lost any emotional attachment for their own people" (p. 223). Of course, what is meant is Andalusi Arabs, not Spaniards, but the confusion of ethnicity with geographical or genetic entities is real. "Spaniards," he says in reference to an Arabic faction that agreed to a face-saving surrender to the Christians, "believe in honor above all" (p. 234). Such ethnic reductionism is one aspect of the datedness of Ashtor's vision that strikes me as resolutely traditional (in sharp contrast to his pioneering research in social and economic history), a summa of classical history of the "Golden Age" of Spanish Jewry (many nineteenth-century secondary sources are cited, with few references to research published after 1950).

The edition is excellent in a literary sense but spotty technically. For example, *fukarah* is repeatedly used for *fuqaha'*, the plural of *faqih* (jurisprudent); Sefardi is rendered as "Spanish" when Arabic-speaking Jews are meant (Andalusi is the correct ethnic denominator); and the *sahib al-suq* is not a police chief.

THOMAS F. GLICK
Boston University

JEFFREY BURTON RUSSELL. *Lucifer: The Devil in the Middle Ages*. Ithaca: Cornell University Press. 1984. Pp. 356. $24.95.

Two features of this book are particularly extraordinary; to understand the author's intentions one must take both into account.

First, although Jeffrey Burton Russell announces a study of "the Devil in the Middle Ages," what he delivers is far more. Indeed, his book verges at times on a general survey of medieval intellectual history. He explores Erigena's metaphysics and Anselm's soteriology only to conclude that the Devil played a major role in neither. He takes pains to reconstruct Dante's cosmology from bottom to top. When dealing with art, drama, and folklore, he follows his stated theme more closely. But the greater part of the book deals with philosophy and theology, which take Russell beyond the confines of diabology into the metaphysics of evil and the overall structure of the cosmos. One might view such passages as necessary background, but frequently the background seems to overwhelm the foreground. Explicit theories of the Devil seem, in some parts of the book, to play an incidental role.

The second puzzle is Russell's mixture of history and theology. The organization of chapters 2 through 10, partly chronological and partly thematic, is in any case essentially historical. In chapters 1 and 11, however, Russell endeavors to persuade the reader that the Devil in fact exists—or, more precisely, that the metaphor of the Devil stands for "a real force . . . actively present in the cosmos urging to evil" (p. 305). In his opening chapter he adduces empirical evidence of criminal malevolence as support for this view. His concluding chapter harks to the authority of revelation and insists that Christian theology cannot abandon the testimony of Scripture and tradition. One might easily see these chapters as needless excursus, unrelated to the historical material sandwiched in between.

Both these features can better be appreciated, however, if one takes the book not as a study in cultural history but as an exercise in historical theology. Russell's central question really is not "How

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did the concept of the Devil evolve in medieval theology, literature, and art?" Instead, he asks, in effect, "Can we coherently account for evil without positing a conscious 'center' that actively opposes good?" He is enough of a historian to grapple with this question on the testing-ground of history. Yet he does not study Anselm, Thomas Aquinas, and others with the neutral gaze of a detached scholar; he engages them in debate. This is clear from his ongoing polemic with the Neoplatonic notion of evil as privation and from his treatment of scholastic realism. The former he rejects as untrue to experience (to which a Neoplatonist might well reply that it is experience rather than metaphysics that needs correction). The latter he finds not so much untrue as irrelevant to experience (doubtless a harder thrust to parry). When he deals with art and literature, what interests him especially is the underlying horror of evil that they betray. They become testimony to those intuitions that philosophy and theology must take into account.

RICHARD KIECKHEFER
Northwestern University

URSULA LIEBERTZ-GRÜN. *Das andere Mittelalter: Erzählte Geschichte und Geschichtserkenntnis um 1300; Studien zu Ottokar von Steiermark, Jans Enikel, Seifried Helbling*. (Forschungen zur Geschichte der Älteren Deutschen Literatur, number 5.) Munich: Wilhelm Fink. 1984. Pp. 233. DM 48.

The great diversity in the forms of historical writing that exists today is usually considered a development of the last two or three centuries. Medievalists, however, have become increasingly aware of the astonishing variety that had already characterized historical writing in the Middle Ages. Ursula Liebertz-Grün studies examples of a particular variant of medieval historiography, namely, three poetical histories in the vernacular written about 1300 in what today is Austria. These works in Middle High German rhymed verse lack the sweep, depth, and idealism of the best medieval German epic poetry. Their function (entertaining and instructing an aristocratic or urban public) and poetic form offered their authors a continual temptation to blur the line between fact and fiction. Nonetheless, these histories do not, like the courtly epic, describe an idealized world or an imagined past. Their authors seem less constrained by the classical canons of a rhetoric of history (however dimly remembered); consequently, they deal with matters the Latin chronicler might have considered inappropriate. This free-wheeling realism makes such works mines of information on the perceptions and attitudes and the political and social realities of the world in which their authors lived—the "other" Middle Ages to which the book's title alludes.

This book consists of three distinct essays, the first two of which were published previously. The first examines the so-called Seifried Helbling, an anonymous author of fifteen poems who looked with a jaundiced eye on Austrian society at the beginning of the Habsburg era; the second is on Jans Enikel, the offspring of a prosperous Viennese family, who wrote a *Book of Princes* and a *World Chronicle*; and the third is a study of Ottokar of Styria, whose *Styrian Rhyme Chronicle* of more than one hundred thousand verses Liebertz-Grün considers an unappreciated classic of historiography. The works of each of these authors are meticulously reviewed with regard to previous scholarship, manuscript tradition, date of origin, authorship, anticipated reading public, as well as the social and political perspectives revealed by the authors. The professed focus of this study is on the social and political structures, the relations between groups and social perceptions and the distribution of power as reflected in what these works have to say about their own world and the past. As a result, there is much here on the growing power of the territorial prince and his relationship with the ruling nobility, on the rise and decline of families of the lesser nobility, on the power of women (or the lack of it), on the Jews and how they were used by the prince and abused by society, and on war, law, the economy, and a host of other topics. This book is based on wide reading and offers a great many enlightening interpretations of the three commentators on the contemporary Austrian society it examines. In the end, though, it provides only three distinct and loosely connected contemporary points of view rather than a generalized and integrated idea of the centers of power and the nature of social relations at a critical point of Austrian history. This is not to deny that Seifried Helbling, Jans Enikel, and Ottokar of Styria present us with fascinating comments on their political and social environment in works whose fables and fictions are often more instructive than the facts they report. Their critical, if not sardonic, view of the late medieval territorial prince and the ruling elites, their eagerness to notice the seamy side of society and its leadership, provide a needed balance to the idealized picture that is given in other narrative sources of the period.

BERNHARD W. SCHOLZ
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NICHOLAS ORME. *From Childhood to Chivalry: The Education of the English Kings and Aristocracy, 1066–1530*. New York: Methuen. 1984. Pp. xii, 260. $22.00.

Nicholas Orme, the author of *English Schools in the Middle Ages* (1973), shows himself with this new book

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an authority on both the theory and practice of late medieval aristocratic education. The book is a detailed study of the childhood and teenage education of the royalty and aristocracy of England from the eleventh to the sixteenth century, tracing this education from the care of nurses, masters, and mistresses until the time came for the youths to leave for further training in a monastery, noble court, or university.

Beginning with the period "Growing Up at Home," Orme discusses the child's education in the nursery and under masters and mistresses, the separate households of royal princes and princesses, the noble children "regularly drafted in to be companions," and the problems and conflicts arising from the planning of careers by parents and friends. We learn of the "education away from home" and the educational practices in royal and aristocratic households, religious houses, schools, universities, and the Inns of Court. But "how far was this practice conceived, described and taught in writings?" (p. 81). The author then discusses the examples of "didactic" writings for clergymen, scholars, and royalty. He examines, for example, the *Didascalicon* of Hugh of St. Victor and the *Disciplina Clericalis* of Peter Alfonsi (d. ca. 1140), whose collection of stories (many from Arab sources) tried to teach wisdom and self-discipline to the clergy, as well as those "mirrors of princes," which were books for kings and princes on how to reign virtuously and successfully. Three important medieval friars (Bartholomew Glanville, Vincent of Beauvais, and Giles of Rome) and their theories are singled out for explanation and evaluation. At various times the discussion comes around to the education of women, but sparse documentation often cuts it short.

Chapters 4, 5, and 6 take up the content of that aristocratic education, and the wide medieval curriculum is found to embrace many skills and values—religious, intellectual, social, artistic, and physical. Our thirteenth-century educationists, for instance, chose those subjects deemed appropriate for each stage of childhood, which, incidentally, was subdivided variously by the theorists. We find out that routines were established, but only in the fifteenth century were they clock-timed and written down. With regard to languages, French was a major aristocratic subject until about the 1350s; with regard to beliefs, the child was taught religion almost before it learned to talk. The term "behavior" conjures up the courtesy literature; Orme examines, for example, the English *Book of Courtesy* (composed after 1450). He also discusses literacy and the skills of dancing and singing, so characteristic of aristocratic education, and in the chapter entitled "Athletics" he covers not only military training but also hunting, hawking, and archery.

The volume is crammed with educational goodies, not least of all, the several synopses of recondite medieval educational treatises. There are good bibliographical notes. This book will probably become a standard work on its subject—and deservedly so.

LOWRIE J. DALY
Saint Louis University

T. H. ASTON, editor. *The History of the University of Oxford*. Volume 1, *The Early Oxford Schools*. Edited by J. I. CATTO. New York: Clarendon Press of Oxford University Press. Pp. xli, 684. $91.00.

This is the first of eight planned volumes in the most ambitious project ever conceived for producing a modern history of one of the great universities. It is not encyclopedic, it does not pretend to be comprehensive or definitive, and its chronological rough edges toward the *terminus ad quem* (Ockham's generation) are so irreconcilable that the editor, J. I. Catto, asks for the reader's forbearance in what is obviously an insoluble, but happily not a serious, problem. If we have science covered for the thirteenth century, law treated to 1380, and arts and theology to 1320, we can trust that the second of the two volumes devoted to medieval Oxford will contain the appropriate compensations.

The well-rationalized scheme of chapters offers a logical development of thematic material and can be divided into three large parts. The first presents the origins and emergence of the university as a corporation and community. A second group of chapters deals with the appearance of the early colleges and their material support and endowment as well as that of the university. Included in this part is an impressive description and analysis of Merton's medieval properties and their management, drawn from the incomparable archives of Oxford's oldest college. The final section turns to the faculties and curricula.

The contributions are extremely rich in detail that does not obscure or overwhelm because the authors have maintained rigorous organization and structure and have provided synthesis and interpretation. We may suspect that the concluding chapter by the late James A. Weisheipl on Ockham and the Mertonians is the bridging for the further exploration of the physical sciences at Oxford that is promised in the second volume.

At first glance, the curiosity of the volume is the chapter on civil law. Brief and almost truncated, it is valuable and interesting on the few questions it addresses, mainly the statutes governing the teaching of civil law. But it is so far short of the desired breadth that it must be labeled the mystery chapter. An explanation of this puzzling situation may lie somewhere within editorial considerations. Or it

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may be found in Leonard Boyle's excellent chapter on canon law where, in order to deal adequately with his subject, he undoubtedly found it necessary to merge the study of canon law with that of civil law so as to put each in its proper context. With remarkable detail and synthetic skill he has shown clearly how Roman law at Oxford began and continued as the handmaiden of canon law.

It is fair to say that most readers will not move through the volume from beginning to end but will read selected chapters. Even if this were not the case, the format of many contributors dealing with related subjects calls for strong support mechanisms; two of these are the index and cross-referencing. Although failure to cross-reference is not always of great importance, it leaves the impression that each of three authors recounting the same story is unaware of what the other two are doing. This becomes serious, for example, in the treatment by three contributors of the St. Scholastica's Day riot of 1355, which resulted in the most extensive royal intervention of the medieval period, the expansion of the chancellor's civil jurisdiction, and the issuance of the "Magna Carta of the medieval university." In discussing this "biggest and bloodiest battle of all between town and gown" (p. 137), one contributor refers to the second but not the third contributor's account of the event and does not give complete page references, while the other two authors in their treatment of the event do not cross-reference at all. This incompleteness is compounded by the absence in the index of any item that might directly lead one to that particular subject. The control of these problems probably lies with a burdened editorial staff, but one hopes that ways are found to strengthen the apparatus of the succeeding volumes.

The question of bibliography is not mentioned in either of the two prefaces. The extensive abbreviation lists of published and unpublished materials help fill such a need. And it may be that the publication of Edward H. Cordeaux and Denis H. Merry's *A Bibliography of Printed Works relating to the University of Oxford* (1968) and the decision in the same year to launch this vast project are not unrelated.

Lastly, it should be said in tribute that, although the scholarly accomplishment represented here owes much to many, very high on the list of those deserving credit should be H. E. Salter and A. B. Emden, whose long and useful lives of work are visible everywhere in this book.

FRANKLIN J. PEGUES
Ohio State University

MODERN EUROPE

ANTONY BLACK. *Guilds and Civil Society in European Political Thought from the Twelfth Century to the Present.* Ithaca: Cornell University Press. 1984. Pp. xv, 280. $32.50.

From about 1940 until quite recently, the historical study of guilds and corporate politics was out of favor. That is changing now, and Antony Black's latest book makes an important contribution to this revival by restoring the theme of corporatism to the prominence it deserves in European political discourse from the Middle Ages to the twentieth century. Ranging from medieval communes and jurists to modern social theory, Black investigates both formal political philosophy and the "spontaneous convictions of everyday life" in an impressive variety of actual historical contexts to re-create the history of European political thought as a dialogue between a "guild ethos" of solidarity, mutuality, fraternity, and friendship and a competing model, grounded in the notions of personal liberty, legal equality, private property, and the competitive free market, that represents the core of European liberalism but that, "for reasons of neutrality," he calls "civil society" (p. xi). Black's book is thus no mere survey of corporatist theories and ideologies, for he is concerned to illuminate that complex dialectic of corporate and liberal values that "formed the moral infrastructure of our civilization" (p. 237) and to contest what he considers the distorted interpretation of liberal values that has come to dominate the political thought of the modern West. The recent "decline of guild values," he asserts, constitutes "an impoverishment of our social being and our social consciousness"; he concludes that "the whole arena of associational life . . . is one which western political thought desperately needs to re-occupy, and to synthesize with its often fanatical concentration on the values of civil society" (pp. 238–39).

Black's belief in the desirability of such a synthesis establishes and circumscribes the framework of his historical analysis. He begins by arguing that corporate and liberal values did indeed develop simultaneously and often in harmony in Italian and German towns from the twelfth to the seventeenth century. Although medieval political thinkers remained reluctant to explore the theoretical implications of the frequently crucial role guilds played in town life, a true "philosophy of the guild" finally emerged in Althusius, who, according to Black, succeeded in integrating concepts of mutuality and reciprocity with those of exchange and contract to provide "perhaps the most substantial exposition of guild ideas ever known" (pp. 131–42). After the Enlightenment's general assault on guilds, modern corporatism bifurcated: whereas older medieval notions of mutuality and fraternity found their way into the nascent trade union movement, a new "romantic philosophy of community," promoting notions of real group personality, *Genossenschaft*,

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and organic solidarity, was elaborated by F. G. W. Hegel, Otto von Gierke, and Emile Durkheim.

Some readers may feel that Black's models of "guild ethos" and "civil society" are abstract constructs serving the needs of ideological prefiguration more than those of historical explanation. His "guild ethos," for example, ought to give medievalists a moment of doubt: although Black makes clear enough his view that medieval guilds were not what Gierke and Durkheim thought they were, his own understanding nonetheless reflects a tendency to romanticize those values of mutuality and solidarity that he posits, *mutatis mutandis*, as constants in the European corporate tradition. This obscures the extent to which, in the Italian cities at least, the experience of guild association rested on a tense balance between such affective values and the ever-present realization of diversity and the potential for conflict among the constituent members of an *universitas* or corporation. It may be true, as Black claims, that some writers, like the Augsburger Clemens Jäger in the sixteenth century, invoked the "guild ideal of friendship" as "a remedy for class strife." But this does not mean that guilds were in fact the "devoted, willing, obedient, friendly" communities that Jäger imagined, nor do such idealizations necessarily "take us straight back to the beliefs and aspirations of the early towns and the medieval guild risings" (pp. 117–18). For, far from being associations of like-minded persons embodying a natural consensus and a moral unity in which individual ambition was muted by common purpose, medieval guilds acknowledged diversity and legitimated competing interests in legal structures that allowed for the representation of the "parts"—whether of single guilds or of guild federations. As the notaries of Bologna put the matter in the mid-thirteenth century, "quos animorum diversitas separat, virtus iuramenti confederat." In short, diversity and competition were built into the language of guild society, at least in its formative centuries.

Objections of this sort, however, should not diminish the significance of Black's forceful demonstration that guilds and corporate notions of politics have played a far more important role in the European political tradition than liberal and Marxist historians, albeit for different reasons, have commonly allowed. One might wish to question Black's implicit assumption that the recent "decline of guild values" is the result of any especially "fanatical concentration" on liberal values and to ask if it was not instead the unhappy but predictable consequence of the romantic turn taken by corporate thought in the nineteenth century and its subsequent distortion in the fascisms of the twentieth. In any event, this book raises fundamental issues that need to be addressed if the historical understanding of European corporatism is to be rescued from the neglect into which it fell after the Second World War.

JOHN M. NAJEMY
Cornell University

JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 4, *Reformation of Church and Dogma, 1300–1700*. Chicago: University of Chicago Press. 1984. Pp. li, 424. $27.50.

Jaroslav Pelikan provides an excellent survey of the history of Christian doctrine from Duns Scotus and William Ockham to Francisco de Suarez and William Ames. The book is impressive, not only for its unfailingly lucid exposition of difficult and sometimes perversely complex theological problems but also for the soundness and solidity of its historical judgments. Although written at an intermediate rather than a beginning level, it is nevertheless accessible to beginners, who will find unfamiliarity with the subject no obstacle to understanding the author's transparently clear explanations.

At the same time readers should be warned that the book is a history of Christian doctrine, not a religious history of the later Middle Ages and Reformation. It examines not what individual Christians believed, thought, and experienced in their particular social settings but what the various Christian churches believed, taught, and confessed. Although individual teachers made indispensable contributions to the debate over what the churches should teach, the author's primary focus is not on individual theologians (though Pelikan discusses at length, as he properly should, the thought of prominent individuals) but on the doctrines themselves in their ecclesial settings. Readers who are interested in late medieval mysticism or in the religious aspirations of the Renaissance humanists will not find much help in this book. The author did not intend them to; he has set for himself a quite different task.

Histories of doctrine are, of course, not a new genre. Several of the most impressive ones, including the multivolume histories by Reinhold Seeberg and Adolf von Harnack, were written in the late nineteenth and early twentieth centuries. Pelikan's history takes its place in this grand tradition. Indeed, it is against the background of these earlier histories of doctrine that his achievement can best be assessed.

Pelikan's discussions of late medieval theology, to take one example, are not as technical as Seeberg's, and there are topics on which the last German edition of Seeberg still provides the best general introduction. On the other hand, Pelikan's sure-footed treatment of late medieval debates over the meaning of the Augustinian tradition reflects the latest historical scholarship and moves far beyond

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discussions in earlier histories of doctrines. It is difficult to think of another general introduction one could recommend to students that deals with such authority and in such meticulous detail with topics as diverse as the late medieval Catholic confession of the immaculate conception and bodily assumption of Mary, the Reformed theology of the covenants, and the Lutheran conception of sin. Neither Seeberg nor Harnack can provide anything like the fair-minded and illuminating explanation of the Reformation debates over the doctrine of the Lord's Supper found in Pelikan's history.

A second difference becomes apparent when Pelikan is compared with Harnack. Both strive for and attain an admirable level of clarity; both command an impressive breadth of primary sources. But, whereas Harnack used the data of Christian history to further his own brilliant and brilliantly flawed thesis about the progressive Hellenization (and therefore progressive corruption) of Christianity, Pelikan remains almost invisible in his book. That is not to say that his history is not graced by original approaches and insights. For example, his treatment of late medieval theology under the rubric of theological pluralism offers a fresh slant on familiar subject matter and sets exactly the right tone for considering the achievements of this remarkable period. Nevertheless, Pelikan's book is less noteworthy for its overarching theses than for its comprehensiveness, clarity, and solid reliability. Pelikan places us in direct contact with the writers he is interpreting. We hear them in their own words, debating on their own terms the issues that were crucial to them. It is just that invisibility of the author behind his sources that makes this book so useful for students of the Middle Ages and Reformation and guarantees this volume, as well as the series as a whole, a permanent place as a standard reference work.

DAVID C. STEINMETZ
Duke University

RAYMOND SCHWAB. *The Oriental Renaissance: Europe's Rediscovery of India and the East, 1680–1880.* Translated by GENE PATTERSON-BLACK and VICTOR REINKING. Foreword by EDWARD W. SAID. New York: Columbia University Press. 1984. Pp. xxiv, 542. $45.00.

Raymond Schwab's *La Renaissance orientale* (1950) has finally been translated into English, although it is hard to imagine for whom this translation is destined. Undoubtedly, the translators and the author of the foreword, two of whom are professors of English, intend to benefit students of literary romanticism and comparative literature. This book can perhaps only indirectly serve that purpose, since it focuses primarily on linguists resident in France in its study of the impact of Indology and comparative linguistics. The titles of the chapters themselves—"The Birth of Linguistics," "Repercussions in Literature," "The Moving Spirits"—reflect the vague, metaphorical character of Schwab's prose, which gets in the way of truly "rational" comprehension. No pretense is made that Schwab, a literary figure who authored poems, novels, and essays, had scholarly credentials. For the 486 pages of text, there are only 13 pages of Schwab's original footnotes plus the translators' clarifications. The 26-page bibliography, which refers mainly to works Schwab considered important to the writing of the book, is updated by the translators with only thirty-two books and articles published after 1950, of uneven quality and importance. Thus, from a scholarly point of view, the work is left much where it was in 1950, with vague references, popularized versions of Oriental names, and an unclear, incomplete bibliography, which inhibited and still inhibits the development of further lines of research. The opportunity to improve on Schwab's work has been lost. Perhaps this improvement would only have been possible if the translation had appeared in Schwab's lifetime, for only he could have reconstructed the sources of the rich, hopefully correct, anecdotal and social historical evidence he recorded.

What remains of value in Schwab's book are the disjointed but mutually reinforcing thumbnail sketches of major and minor figures in the eighteenth- and nineteenth-century history of European Orientalism, which form a composite portrait of panoramic sweep. Valuable, too, is Schwab's breadth of vision when he recounts the fields and thinkers on which the new Orientalism was a major influence. He ranges lightly and synthetically over literature, religion, history, politics, philosophy, racial theory, biology, and geology; in Gobineau, Nietzsche, and Tolstoy, Schwab finds food for thought.

The publisher is to be complimented on the layout and index of the book. This includes the cover; although its nondescript, quasi-Oriental typography only hints at the contents, it is, nevertheless, an improvement on the nude female figures that have decorated many books on the influence of Orientalism and the exotic, however relevant such figures may have been to male interest. Even the cover of the original French edition, which contained a photograph of a sculpture of a (seemingly nude) Hindu demigoddess, almost caused the book to be confiscated as pornographic when the present reviewer tried to bring it into East Germany.

JOAN LEOPOLD
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TRYGVE R. THOLFSEN. *Ideology and Revolution in Modern Europe: An Essay on the Role of Ideas in History.* New York: Columbia University Press. 1984. Pp. xv, 287. $30.00.

The full scope of this study becomes clear only when both title and subtitle are taken into account; it deals with two issues: the one is a general consideration of the role of ideas in history; the other tries to give substance to these theoretical considerations by examining the importance of ideas in the development of modern revolutions.

The first of these issues is treated in the introductory chapter as well as in the second part of the book. These chapters are of a theoretical nature explaining what modern historians have said about the role of ideas in history (particular attention is given to Otto Hintze and the *Annales* school) and in what form the problem appears in the modern social sciences; the latter discussion focuses on modern anthropology and on the neo-Marxists.

These somewhat abstract discussions frame what might be regarded as the centerpiece of the book. On the basis of a study of the French Revolution, the Russian Revolution, and the origins of fascism, the author tries to analyze the relationship between intellectual demands and events in the political and social sphere—to what extent ideas can be regarded as having determined the aims of the movements and even the course of events in France, Russia, and Italy.

To establish the connection between ideas and actions in one of these revolutions would be an immense task; to study the issue in all three of these revolutions within a single book seems impossible. The author is certainly aware of this difficulty and has made no attempt at presenting results of his own research. He explains the views that are expressed in recent historical studies of these revolutions. The author's knowledge of this literature is admirable, and even the expert will find references to writings of which he had no knowledge. It cannot be denied, however, that these summaries of the views of a great variety of authors do not make for easy reading, and sometimes the book is more a bibliographical study than a historical analysis. Nevertheless, there is no doubt about the stimulating character of this careful probing into a complex issue.

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ALLAN MEGILL. *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida.* Berkeley and Los Angeles: University of California Press. 1985. Pp. xxiii, 399. $24.95.

How does one write a history of thinkers who believe that texts must be read ahistorically without appearing to take sides against their thought? How can the historian deal empathetically with prophets who proclaim with a savage glee the collapse of historicism?

These are difficult questions, confronted bravely by Allan Megill. Traveling the *via regia* of intellectual history defined by H. Stuart Hughes in *Consciousness and Society* (1958), he takes on the task of explaining the avant-garde in Germany and France since Friedrich Nietzsche, in particular, a segment of the avant-garde that has debunked the methodological premises of intellectual history itself. He could have emulated his subjects and engaged in a postmodernist shredding of their texts. So he does, in spots. But he also borrows various tricks from traditional *Geistesgeschichte*, as he follows the path from Nietzsche's announcement of a great spiritual crisis in Western civilization through the woodsy quietism of Martin Heidegger and the intellectual terrorism of Michel Foucault to the dense self-parody and oddly rabbinical nihilism of Jacques Derrida.

Megill makes it an exciting journey. His dominant themes are a consciousness of the failure of historicism (here construed as a sense of directionality in history) and its myth of progress, together with an ever-accelerating tendency to replace the search for objective reality with an assertion of the primacy of art and language. Yet in the process, writes Megill, art and language have acquired an ontogenetic charge, and reality has returned in the form of texts that do not encode an external truth but are seen as truth in themselves.

On the whole, Megill's enterprise succeeds. He wisely refrains from holding his prophets to the rules of a rationality they scorn. At the same time, he enlarges our understanding of their works (whether the authors would approve or not) by importing extratextual evidence from their lives and times. Of particular value are the chapters on Derrida, which argue that the French philosopher has deconstructed the claims of aestheticism as well as of science and historicism, possibly clearing the ground for a new era of truth seeking. One is reminded of Michael Ryan's recent *Marxism and Deconstruction* (1982), which finds the basis for a critical, open-ended Marxism in Derrida's Gödelesque doctrine of undecidability.

Megill's only serious fault is to bloat the importance of the last three members of his "gang of four." One cannot challenge the paramount role he assigns to Nietzsche, but Heidegger, Foucault, and Derrida are no more than convenient illustrations of the irrationalism at large in the twentieth-century European mind. As Megill himself admits in a few brief asides, the century has been thick with crisis-

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mongers, many just as original and influential as the three he selects.

At all odds, the movement in thought they represent has now more or less exhausted its possibilities. The career of Derrida marks the end of the "new beginning" made by Nietzsche (p. 1). But whatever happens from this point forward will owe much to the demolition work that Derrida and so many of his contemporaries have performed with uncommon brio.

W. WARREN WAGAR
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COLIN LOADER. *The Intellectual Development of Karl Mannheim: Culture, Politics, and Planning*. New York: Cambridge University Press. 1985. Pp. ix, 261. $34.50.

It may be too early to speak of a Mannheim renaissance, but signs of it are already in evidence. One might cite, for example, the recent publication of the Hungarian-born sociologist's uncompleted manuscripts, *Structures of Thinking* and *Conservatism*, or the critical interpretations of his work jointly authored by David Kettler, Volker Meja, and Nico Stehr. Joseph Gabel in France and Éva Gábor in Hungary have lobbied effectively in behalf of Mannheim, and A. P. Simonds, the author of an outstanding study of his thought, has called him one of the most creative—and underrated—thinkers of this century.

To be sure, Mannheim was never completely forgotten. His name appears repeatedly in connection with such inexhaustible subjects as ideology, utopia, the sociology of knowledge, intellectuals, and democratic planning. Why, then, the neglect? One reason, perhaps, is Mannheim's noncommittal politics. Beyond that, many have been put off by his purported failure to overcome relativism and by his eclecticism. In a fresh effort to dispel such prejudices, Colin Loader argues that Mannheim's career—in Hungary, Germany, and England—should be seen as a "dynamic totality" informed by the "concept" of the "refugee"—that is, the application of one culture's lessons to another.

In presenting his case, Loader divides Mannheim's intellectual development into five periods, two of which are transitional in character. The major, synthetic, periods, he tells us, may be defined by their "centers of construction": cultural philosophy, politics, and democratic planning. The author discerns several continuities underlying these shifts in focus, including Mannheim's lifelong interest in the problem of alienation. Unfortunately, the other abiding concerns he identifies are unexceptional and hence uninstructive: the responsible individual, the will to change, the orientation of the individual in the changing modern world. Worse, the methodological apparatus that he employs assumes such Frankensteinian dimensions that it obscures more than it reveals. The endless series of constructions, constellations, types, stages, categories, and concepts finally produces a narcotic effect and even weakens our hold on simple identifications: Max Weber is an "ideal type," the three countries in which Mannheim lived are "sociocultural contexts," theoretical problems constitute a "dynamic hierarchy." We can only agree when Loader writes that "the very 'arbitrary' nature of my methodology should help to convince the reader . . . to return and consider [Mannheim's] texts" (p. 186).

But, even if the methodology were less surrealistic, it would mislead, because the logic of a historical totality compels Loader to exaggerate the importance of the final (planning) stage and because the entire approach is ironically at odds with Mannheim's own view: that ideas cannot properly be understood without reference to the existential conditions of those who think them. Loader insists that he is considering Mannheim only as "an intellect." He does make some contribution in this respect, although he passes over in silence three thinkers whose ideas greatly influenced that intellect: Béla Zalai, Paul Szende, and Martin Heidegger. More important, however, it was a person, not simply an intellect, who wrote *Ideology and Utopia*.

LEE CONGDON
James Madison University

JAN DE VRIES. *European Urbanization, 1500–1800*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1984. Pp. xvii, 398. $28.50.

Once in a while a book appears that begins the reshaping of a field of historical study. Once in a far longer while, a book like this one comes along and itself carries out much of the reshaping. Jan de Vries's history of urbanization demonstrates what many have been waiting for in doubt: that the rigor of the "new" economic history and the sensitivity of historians' history can after all make a balanced marriage. De Vries himself surpasses other scholars in skill, knowledge, resources, and candor of style, which he can afford since he has so much more to offer in the quantitative analysis of early modern urban history than has ever been offered before.

All in all, de Vries outflanks historians, who have studied cities comparatively and singly, by showing that cities are comprehensible only when considered as parts of a transnational system; geographers, by his facility with their techniques; and urban specialists, by revealing the unhistorical character of their

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celebration of a new urbanism since the nineteenth century. That celebration proves to be misplaced. The urban system was largely a seventeenth-century creation, the big disjunction coming between 1600 and 1650, when a polynuclear city pattern was upset and gave place to a centering in the northwest. Industrialization in the period 1750–1850 at first reinforced the shift toward the Atlantic; only later, with the emergence of the *Ruhrgebiet*, did it reverse this shift, and the stock of big cities slightly increased.

The exciting modifications to European economic history have for some years been arriving "from below," so to speak, from the early modernists. Protoindustrialization was among these novelties, and it is interesting to note that de Vries thoroughly establishes its significance despite recent attacks on the concept. Other features of his study are that he sees city dwelling and urban industrialism as further options within a universe of agriculture and rural industry and that the urban system was a precondition rather than a creation of urban industrialism.

The author's use of transformation matrixes, urban "potentials," and the like will stun many historians—for this is a stunning book—but is not *au fond* impossibly complicated. A true scholar, de Vries does not apply, in the showy cliometric way, mathematical methods to other people's historical labors; rather, he chooses whichever method, statistical or historical, seems appropriate to the task. We learn as a result the dimensions and fluctuations of cities throughout Europe and the Europeanized regions and a great deal more than before about the mechanics of their chronological, spatial, and demographic reshufflings. If Jan de Vries cannot persuade history graduate schools to cease their Luddism over mathematics, no one can.

With respect to migration and demography much remains to be done, notably, testing further Sharlin–van der Woude's assault on the so-called law of urban natural decrease. Also, cities have yet to be examined as capital investments and physical fabrics, since this book instead conceives of them as vessels defined by the populations they contain. The database on population sizes will be de Vries's longest-lasting contribution, but the methods and conclusions he derives from manipulating the base will also inspire for a long time to come. The methods de Vries uses should now be applied to the study of other continents and other periods. This is social science history at its best.

ERIC JONES
La Trobe University

CHRISTOPH M. KIMMICH, editor. *German Foreign Policy, 1918–1945: A Guide to Research and Research Materials*. (Guides to European Diplomatic History.) Wilmington, Del.: Scholarly Resources. 1981. Pp. ix, 293. $17.50.

ROBERT J. YOUNG, editor. *French Foreign Policy, 1918–1945: A Guide to Research and Research Materials*. (Guides to European Diplomatic History.) Wilmington, Del.: Scholarly Resources. 1981. Pp. xv, 242. $17.50.

ALAN CASSELS, editor. *Italian Foreign Policy, 1918–1945: A Guide to Research and Research Materials*. (Guides to European Diplomatic History.) Wilmington, Del.: Scholarly Resources. 1981. Pp. xi, 271. $17.50.

GEORGE W. BAER, editor. *International Organizations, 1918–1945: A Guide to Research and Research Materials*. (Guides to European Diplomatic History.) Wilmington, Del.: Scholarly Resources. 1981. Pp. xi, 260. $17.50.

During the past twenty years the opening of the principal West European archives has transformed conditions for the study of interwar diplomacy. Not so long ago, the scholar seeking to get beyond the published documents had to make do with the captured German microfilms, American records of indifferent quality, and the limited gleanings from private papers that became available by happenstance. Persistence and a high tolerance for frustration figured as the twin underpinnings of research. Now an academic lifetime will not suffice to explore every available source. D. H. Thomas and L. M. Case's *New Guide to the Diplomatic Archives of Western Europe*, though last revised in 1975, does not offer sufficiently detailed and current information on the period after 1914 to enable the historian to budget his time closely. Accordingly, Christoph M. Kimmich, Robert J. Young, Alan Cassels, and George W. Baer have done the profession a signal service by providing reliable and comprehensive guides to the diplomatic archives of Germany, France, Italy, and international organizations from 1918 to 1945.

These straightforward and uniformly sensible volumes do not claim to offer a methodological Baedeker for practitioners of the "new diplomatic history." They aim to facilitate research on the making of high policy. They give, at best, secondary emphasis to sources on parliaments and pressure groups, on the formation of public opinion, and on domestic constraints generally, and series editor Kimmich has reserved analysis of business archives for a subsequent volume on international economics. Yet the authors cast their nets well beyond the bureaucratic jurisdiction of the foreign ministries, and they describe the relevant sources for all government agencies that directly influenced foreign policy.

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The volumes follow a standard pattern. They each begin with an essay on the foreign ministry and the making of foreign policy in the country concerned. They then denominate the principal archives, both official and private, with their addresses, holdings, regulations, and (when feasible) opening hours. They also report on specialized research libraries and newspaper collections and offer references to the more detailed extant guides relating to particular institutions. The final two-thirds of each volume comprises a select bibliography covering reference works, documentary collections, dissertations, and memoir literature as well as the classic secondary works. Each listing has its idiosyncracies, but all succeed in providing a fundament that will orient serious students without yielding to an obsessive comprehensiveness that might make the guides unwieldy. Those who lug these volumes from one repository to another will offer thanks for such restraint.

Kimmich undertook in one respect the least daunting assignment. He could draw on the admirable directories already published by the leading German archives and by such institutions as the Institut für Zeitgeschichte. He has not, however, limited himself to summarizing those guides; he has added information on sources for German diplomatic history available abroad, so that his book offers something new even to scholars in the Federal Republic. His introductory essay on the administrative history of the *Auswärtiges Amt* between the wars constitutes a model of its genre. The list of key diplomats at foreign postings and of major ambassadors accredited to the Reich serves as a helpful reference. (The other national volumes also include this feature; the League of Nations volume, unfortunately, omits it.) Some readers will question the criteria according to which Kimmich constructed his bibliography. The publications on Weimar and Nazi foreign policy are so extensive that he faced admitted space constraints. But he appears to have included some outdated works at the expense of more recent ones and to have slighted foreign economic policy.

Young faced the opposite problem from that encountered by Kimmich. The published guides to French ministerial records for the interwar period remain sketchy (although they are improving), and the inventories publically available at the archives do not contain everything. One must frequently seek permission to consult sensitive files. It would be unreasonable to expect Young to emulate the candor of Edmund Wilson, who composed his classic description of working conditions at the Bibliothèque nationale only after resolving never to return there. Yet Young is explicit enough: "Rarely is one told, without much prompting, of all the material that ought to be seen for one's project. The art is to discover what exists for oneself, then to make a case for being allowed to use it" (p. 40). Young generously shares his laboriously acquired personal knowledge on the War Ministry, and he provides competent, if not wholly comprehensive, listings for the Archives nationales, Bibliothèque nationale, Quai d'Orsay, and the various depositories for private papers; his inventory for the Finance Ministry appears less full. He makes a valiant effort to trace administrative changes in the Foreign Ministry in light of his well-founded view that this is an attempt to "tether a broomstick." His bibliographical section includes a particularly thorough listing of recent dissertations, memoir material, and articles; to make room for this, he has restricted the data on monographs and eliminated descriptive annotations.

Cassels provides a splendid guide to the Italian records. The most demanding reader could wish for nothing more. The solitary scholar could assemble such comprehensive information only with difficulty, and Cassels credits numerous archivists and colleagues with aiding his investigations. The many outstanding features of this volume include detailed references to material on foreign affairs in all Italian archives and major archives abroad; a review of relevant archival legislation governing the use of such material; a list of significant diplomats with indications whether they left memoirs or papers; an inventory of collections still in private hands (evidently the result of much skillful detective work); a description of the main Italian newspapers under the Fascist regime and their political orientation; and a bibliography offering refreshingly frank evaluations of published works. This volume will at once satisfy specialist exigencies and make it substantially easier for nonexperts to use Italian materials. Regrettably, Cassels had no opportunity to report on the Vatican archives, since these remain closed to laymen for the period after 1903.

Baer's volume on international organizations stands as the most uneven of the works under discussion; it nevertheless won the Outstanding Academic Book award of its year. The bad news is that Baer devotes no more than ten pages to a sketchy outline of League of Nations archival holdings, for which no published guide exists. The good news is that political historians, with the exception of those studying specialized topics like disarmament, refugees, and minorities, will be able to eliminate Geneva from their archival itinerary. Baer confirms that the League did not play much of a role in the great political issues of the era (p. 82), and this undoubtedly explains the patchy quality of its records. Social historians will find rather more of interest, not merely in the League and United Nations records but also in the files of the numerous specialized agencies to which Baer provides references. Old-fashioned types who think that power still mat-

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ters will wonder what in the Geneva atmosphere prompted Baer to furnish details, for example, on the World Intellectual Property Organization but to pass over the Bank for International Settlements in silence. To Baer's credit, he provides an outstanding annotated bibliography, organized topically, that rescues many useful monographs from oblivion.

In the course of their labors these four authors may have wondered at times if they had embarked on a thankless task. They can begin to count the votive candles. Every research scholar whose work touches on diplomacy or who advises graduate students in the field will come to regard this entire series as an indispensable working tool.

STEPHEN A. SCHUKER
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JOSEPH R. WASHINGTON, JR. *Anti-Blackness in English Religion, 1500–1800.* (Texts and Studies in Religion, number 19.) New York: Edwin Mellen. 1984. Pp. xix, 603. $69.95.

Throughout this unusual book, Joseph R. Washington, Jr., insists on a fundamental distinction between "antiblackness" and "anti-Blackness." He uses "antiblackness" to denote the many negative aspects of English culture and especially its Christianity: the devil, witchcraft, sin, and especially the imageries of darkness in Anglo-biblical culture. He insists that this rigidly held and powerful view of the world was extended to the people whom the English called "black" or "negro." Hence, pejorative thoughts about human nature in general came to be fastened on a particular people: the Africans enslaved by the English in America. Thus, the central thesis is that a deep-seated religious and cultural stance of antiblackness made the English and their overseas descendants fundamentally, ineluctably "anti-Black"—deeply opposed to "Black" people.

The book is sustained especially by the use of an enormous range of source materials, including Shakespeare, Richard Hakluyt, Samuel Purchas, George Fox, Cotton Mather, Samuel Sewall, George Whitefield, John Woolman, Charles Grandison Finney, and William Lloyd Garrison. As these names suggest, the book is more about early America (well past 1800) than about England. Yet it is the roots of antiblackness and anti-Blackness that most interest the author. His discussion of attitudes toward Gypsies and the story of Ham in the sixteenth century constitutes the most interesting part of his study. As the book moves forward chronologically, it becomes more firmly rooted in wide reading of the writings of important individuals, but it also becomes less innovative and less illuminating.

The book's emphasis on the inherency of cultural postures is persuasive, although its steady focus on religion as such has pronounced limitations for an understanding of the history of attitudes and ideas about race and about English Christianity as it moved into the New World. The author pays virtually no attention to social circumstances, a disposition that leads to considerable confusion about American Indians in a chapter entitled "The Brown Connection between Black and White." Almost as asides, he greatly exaggerates the importance of the slave trade to the Puritan New England economy and the number of Africans caught up in the Atlantic slave trade.

Throughout, the focus is on known writers rather than on such sources as newspapers and statutes. The author finds anti-Blackness infecting such men as Cotton Mather and Samuel Davies, both of whom were insistent on the equality of souls despite color. He argues persuasively that even John Woolman "missed altogether the depth of English anti-Blackness and its meaning" (p. 470). He says that the vision of "Reverend Woolman" went only so far as to see slavery as the font of anti-Blackness when in reality that disease was a much more fundamental cultural malady. He accords that "saintly" Quaker "uncanny insight" but at the same time "cultural myopia" (pp. 469–470).

Unfortunately, the book is written in a style that is bound to weaken its impact. The prose is often so murky and convoluted as to be downright impenetrable. Too many sentences have to be reread several times in order to divine their intended meaning. What, for example, is one to make of the following paragraph? "It is not trivial to inquire into the phenomenon of anti-Blackness by means of an exploratory question. Before their elevation to the larger-than-life status was the die cast respecting anti-Blackness in the Puritan synthesis of Western Christianity and the Shakespearian synthesis of its secular philosophies? Did they generate such respect (in retrospect or prospect) until their descendants were forced to shift between Calvinism and Stoicism into the fitful synthesis of universalism and moralism and slip from complacency into indifference in the spirit of disinterested will to the good?" (p. 63). The author's postscript promises a sequential study: the present book, he writes, "is the end of the beginning of an investigation into the meaning of the irreversible and inextricable sacred-secular-race triplet power and powerlessness, the cause and effect of English bearing and the Americanization of its public-private-parochial design" (p. 539).

With such words the author's thoughts have clearly run ahead of his control over their expression. The footnotes, bibliography, and index also suffer from sloppiness. They are riddled with errors in titles, names, and page numbers. The text itself, including its many very long block quotations, has too many errors of typography and wording. Be-

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cause the book is learned and perceptive, these limitations are especially unfortunate. But most of all, despite the inherent interest and importance of its subject, it is just plain difficult to read.

WINTHROP D. JORDAN
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MARTHA TUCK ROZETT. *The Doctrine of Election and the Emergence of Elizabethan Tragedy*. Princeton: Princeton University Press. 1984. Pp. ix, 329. $32.50.

As the title suggests, Martha Tuck Rozett's primary intention is to show how Elizabethan playwrights "reacted to their audience's expectations [and] how these expectations were at least partly determined by the preoccupations of the age" (p. 4). After reading this impressive book, most readers will undoubtedly conclude not only that Rozett reached her goals but also that her work indicates how much historical scholarship can still contribute to modern literary criticism.

Rozett demonstrates how pervasively and intricately Shakespeare, Marlowe, and their contemporaries were influenced by Puritan thinking. Appropriately, Rozett concentrates on the doctrine of election, that grimmest of binary oppositions that assumes different names in different times and places but ultimately amounts to the basic distinction provided by modern psychologists between the self and the "other." Without ever allowing the text to become repetitious, Rozett keeps this antithesis constantly in play, perceiving it to be not only the principal interest—and anxiety—of the age but also a powerful determinant that accounts for much of the action, psychology, and structure of the drama.

The stark opposition between the elect and the damned is most evident in Elizabethan morality plays and the early tragedies in which the hero is clearly elect and the villain reprobate. In the more sophisticated later tragedies, however, disturbing ambiguities appear and blur the doctrinal antithesis by presenting tragic heroes who embody characteristics of both the righteous and the rejected "other." This characterization is particularly noticeable in revenge tragedy, where the protagonist is, paradoxically, victim and villain, objectifying attitudes and motives stereotypical of the Puritan scourge of God and the presumptuous reprobate. Like the overreacher, Rozett contends, they are "embodiments, however perverse, of [the audience's] own aspirations, and of the boldness and assurance that were both celebrated and condemned by the culture in which they lived" (p. 264). In view of this thesis one can more easily understand why the audience is inclined to identify, at least partially, with characters such as Tamburlaine and Richard III who are a far cry from the virtuous protagonists of the morality plays and the pastoral comedies.

This book is not flawless. It would be even more instructive if at the outset Rozett had devoted a separate section to the Puritan doctrine of double predestination, summarizing the various deviations that one finds in English Puritanism. Occasionally, she sees Geneva Calvinism where it is not to be found, as, for example, in *Paradise Lost*. Rozett maintains that "Adam and Eve deliberately choose a course of action to which they are predestined by a God whose motives are veiled in mystery" (p. 292), whereas the text states that Adam and Eve were "sufficient to have stood, though free to fall" (3.95–119) and the *Christian Doctrine* argues against a doctrine of selective election insisting that God predestinated all mankind to salvation (1.4).

At times she exaggerates the degree to which the Elizabethan audience would have been influenced by the predominant ambiguities inherent in the doctrine of election. It is certainly true that English Puritans considered boldness and assurance marks of salvation and that, accordingly, they would have repudiated aspiration, ambition, presumption, and pride at first sight. But it is greatly to be doubted that Shakespeare's audience would have considered Macbeth at the close of the play "a human being, who was, and was not, an image of themselves" (p. 300).

Misconceptions like these are, however, few and far between. What matters much more is the overall quality of the text. Rozett has written an admirable book that deals with a large number of Elizabethan plays, offers one valuable insight after another, and is extremely well written.

JOHN X. EVANS
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HANS S. PAWLISCH. *Sir John Davies and the Conquest of Ireland: A Study in Legal Imperialism*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1985. Pp. x, 244, $42.50.

A growing body of scholarship is devoted to the process by which the Hiberno-Britannic relationship reached the dire state from which so much modern distress has flowed. In the last decade Brendan Bradshaw has argued powerfully for the central importance of the 1530s and a series of changes he has described as "constitutional revolution," while Nicholas Canny has drawn renewed attention to the mid-Elizabethan years as the critical watershed between the loose medieval tie and its more intrusive and structured modern successor.

Hans S. Pawlisch has now illuminated a slightly later period during which the English constitutional

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and military triumphs of the sixteenth century were translated into a legal "settlement" of enduring consequence. The period was the reign of James I and the architect of the plan was Sir John Davies, attorney general for Ireland from 1606 to 1619. The title of Davies's best-known tract defines and encapsulates his conclusions about Ireland. He called it "A discovery of the true causes why Ireland was never entirely subdued and brought under obedience of the Crown of England, until the beginning of his Majesty's happy reign" (1612). The key phrase is "entirely subdued," for the medieval relationship neither required nor envisaged any such perfect subordination. But for Davies, and probably for the king, the old relationship was anathema: "For though the Prince doth bear the title of sovereign lord of an entire country . . . yet if there be two-thirds parts . . . wherein he cannot punish treasons, murders, or thefts unless he send an army to do it; if the jurisdiction of his ordinary courts of justice doth not extend into those parts. . . : if he have no certain revenue, no escheats or forfeitures, out of the same, I cannot justly say that such a country is wholly conquered" (in H. Morley, ed., *Ireland under Elizabeth and James I* [1890], p. 219).

Davies's solution to this problem was an English lawyer's field day, what Pawlisch calls "the scenario of an English government deliberately using judicial procedures and mechanisms to consolidate its hold over Ireland" (p. 35). Because the Irish Parliament was still dominated by Catholics of doubtful fidelity to the cause, judge-made law was a necessary alternative to statute, and Davies contrived to revolutionize the island's legal system by the promulgation of judge-made law, borrowing conveniently from civil law the doctrine that "conquest eliminated all prior and current rights to property and rule on the part of the conquered" (p. 169). Davies thus provided both the rationale and the machinery for replacing a medieval relationship in which insular autonomy was only loosely abridged by homage to the English monarchy with a "modern" imperial framework in which insular custom was broken and replaced. Clearly, it represented a momentous change.

Pawlisch has made an important and incisive contribution to Anglo-Irish history. There are a few minor errors, the most notable of which elevates the infamous Miler McGrath to the archbishopric of Armagh from his more modest actual station at Cashel (p. 93). In the concluding chapter, "Sir John Davies, the Ancient Constitution and Civil Law," he strays somewhat from the Irish theme to quarrel with J. G. A. Pocock and those who emphasize the insularity of the English legal tradition. For Pawlisch, Davies illustrates the English capacity for eclecticism, and the author warms so energetically to the argument that Davies's significance for Ireland is briefly obscured by the shot and blast. But it would be unjust to tax the author for his preoccupation with legal history when it is in large part his sedulous, intelligent cultivation of that subject that has enabled him to bring new insights to Irish history.

KARL S. BOTTIGHEIMER
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ROBIN D. GWYNN. *Huguenot Heritage: The History and Contribution of the Huguenots in Britain*. Boston: Routledge and Kegan Paul. 1985. Pp. xii, 220. $34.95.

This book appears on the tercentenary of the revocation of the Edict of Nantes and the centenary of the foundation of the Huguenot Society of London, which has published much of the scholarly work on the role of French Protestants in British life. Director of the 1985 national commemoration, Robin D. Gwynn has produced a history that wavers—sometimes happily, sometimes unhappily—between the academic and the semipopular.

Huguenot Heritage is of a genre as difficult to review as it is to write. On the one hand, its impulse and its context are such as to promise the glorification of the Huguenot heritage; there must be that name-dropping of Huguenot descendants and that assertion of Huguenot patriotism that are so familiar from similar works on Jews in England or Italians in the United States. On the other hand, for more than fifteen years Gwynn has been publishing cogent, detailed articles on Huguenot settlement patterns and ecclesiological problems in Tudor-Stuart England; his penchant has been for the low-key, thorough review of evidence. His style, in fact, is curiously at odds with his subject, and the book comes to seem oxymoronic: pedantic but superficial in his chapter on crafts and trades, dryly overwrought in his discussion of the Huguenot contribution to the defeat of Louis XIV's France.

Gwynn concentrates on the late Tudor and Stuart eras during which the Huguenots in England were a particularly coherent (if constantly bickering) immigrant group. He argues that the presence and publicity of Huguenots as exemplars of what could happen to Protestants in a country ruled by a Catholic monarch "helped form the climate of opinion in which it was possible to dispossess James II without bloodshed during the Glorious Revolution" (p. 5). He argues also that the professional military expertise of five to six hundred Huguenot officers and the especial zeal of ten to twelve thousand Huguenot soldiers, as well as the financial resources of wealthy Huguenot merchants, were important if not crucial to the strategic defeats of French forces between 1690 and 1712. Rebutting Warren C.

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Scoville's *The Persecution of Huguenots and French Economic Development, 1680–1720* (1960), Gwynn suggests that, if one considers what the allies gained at French expense, the Huguenot evacuation of France was indeed a heavy blow to the French economy and a significant factor in the rise to preeminence of English (and Dutch) finance.

With 160 pages devoted to demonstrating the Huguenot contribution to a financially sound, politically secure, broadly Protestant England, there remain only 16 pages in which to trace the later process of assimilation. Here Gwynn is at his weakest, as if the fading away of a distinctly Huguenot minority were too sad to bear a finely critical analysis. The genre itself makes such analysis awkward. So much effort has been spent to bring into focus "a seriously neglected minority" (p. 2) that the group's eventual assimilation must be as mystifying as it may be distressing. A more detailed study of it would work against the very point of the book as part of the celebration of the Huguenot heritage.

There are no other modern surveys of the Huguenot role in English life. (Jon Butler, however, has recently published a history of Huguenots in America.) This survey, admirably clear and concise, may serve lay readers, undergraduates, and scholars alike, but none will be wholly pleased.

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ANGUS MCLAREN. *Reproductive Rituals: The Perception of Fertility in England from the Sixteenth Century to the Nineteenth Century*. New York: Methuen. 1984. Pp. viii, 206. Cloth $25.00, paper $12.95.

This is Angus McLaren's third book on the fertility debate; this time, however, he shifts his focus from the nineteenth-century debate itself and concentrates on the actual birth control practices during the early modern era in England. In all his books McLaren unequivocally rejects the view that conscious fertility planning developed only recently—in the eighteenth, nineteenth, or twentieth century, depending on the theory. Instead, he attributes to preindustrial people the same birth control motives that we take for granted in our modern era and that anthropologists contend exist currently among primitive peoples. His purpose in writing *Reproductive Rituals* is to "demonstrate simply the ingenuity and perseverance with which men and women of earlier generations sought to control fertility" (p. 149). To what extent is McLaren successful in this demonstration?

Armed with the methodology and insights derived from anthropology, McLaren marshals a plethora of diverse data to establish that the early modern English controlled their fertility in a fashion similar to that of contemporary primitive people. Resort to the anthropological method allows the historian to apply a new mind set to the past; in this case it helps uncover evidence previously ignored or undervalued. Thus, although historians have long known about certain practices and beliefs—for example, that herbs, potions, astrology, or other forms of "magic" were used to induce fertility or abortion, that early moderns married late, that women who nursed their infants were less likely to become pregnant during lactation, and that postpartum sexual abstinence was generally prescribed—they did not infer that birth control considerations were the underlying basis.

What McLaren tells us about the birth control policy of abortion, as described in the preindustrial literature, offers much food for thought. In England before 1803 abortion was legal. In fact, a more permissive attitude toward the early months of pregnancy had allowed religious men and moralists to accept "abortion" until the female experienced "quickening" or, in Catholic countries, until "ensoulment" occurred, somewhere between the fortieth and eightieth day, depending on the sex of the child (p. 117). Before the nineteenth century, abortive techniques were allowed for what was regarded generally as the restoration of a woman's menses. McLaren's chapters on abortion are sufficient to make this book indispensable reading, although more research is needed to explain why abortion became illegal in England.

In the final analysis, some may not be convinced that people in the early modern period practiced fertility control; objections can be anticipated that the use of contemporary anthropological data to interpret a society from a different time and with a different history is an anachronistic application of current values to the past. Even McLaren admits that the success of anthropology in uncovering the contraceptive strategies of primitive families lies in achieving "an intimate view of the issues of conception and birth" (p. 3). The cultural anthropologist obtains such a view through observation and conversation with the participants, methods denied the historian. The historian compensates for this by using diaries, letters, and recorded observations of contemporaries when available. That McLaren avails himself of such sources contributes to the compelling nature of his thesis. But the question persists: are advice books, literary sources, religious texts, and church penitentials satisfactory evidence that families actually practiced birth control? Two responses can be made: the first is that most knowledge is acquired through such circumstantial evidence, requiring the scholar to infer his conclusions, and the second is that the social historian's inquiry into the intimate conditions of the past will always be hampered by his inability to obtain more direct

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evidence and by the general lack of intimate data. Considering these inherent limitations, McLaren has written a well-researched, convincingly provocative book that informs, stimulates, and contributes significantly to the present debate about population control.

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JAMES E. FORCE. *William Whiston: Honest Newtonian.* New York: Cambridge University Press. 1985. Pp. xxiii, 208. $37.50.

William Whiston was born in 1667 at Norton-juxta-Twycross, Leicestershire. Admitted in 1686 to Clare Hall, Cambridge, where he specialized in the study of mathematics, Whiston took his B.A. in 1690 and an M.A. in 1693, the year of his ordination into the Anglican church. He soon returned to Cambridge to continue his mathematical studies as well as to delve more deeply into the new mechanical philosophy. Although he wrote and published copiously during a life that spanned eighty-four years, his most widely acclaimed and enduring book also happened to be his first: *A New Theory of the Earth* issued from the press in 1696, before its author had reached the age of thirty. Whiston postulated that the earth had been formed from the atmosphere of a comet and that all major changes in the planet's history are attributable to cometary action. Not satisfied with a mere attempt at scientific explanation, the author equated the earth's early formation and physical development with "the Mosaick History of the Creation," that is, with the Old Testament Book of Genesis. Above all, Whiston sought to challenge the deistic thesis set forth in Thomas Burnet's influential *Sacred Theory of the Earth* (1681–84), namely, that the Mosaic account of creation is a "meer popular, Parabolick, or mythological relation."

Not the least of those impressed by Whiston's scientific and theological endeavors was one Isaac Newton, Lucasian Professor of Mathematics at Cambridge and author of the recently published *Principia Mathematica* (1687). Whiston had attended certain of Newton's lectures, and their passing acquaintance gradually developed into friendship—or at least one of the closest imitations of it that the aloof and censorious Newton would allow. In 1696, the year the great man left Cambridge for London to become warden of the mint, he saw to it that Whiston was brought in as his substitute, with the full profits of the Lucasian chair. In December 1701, a year after his advancement to the mastership of the mint, Newton resigned his professorship in favor of his protégé.

The compatibility of the scientific and religious thought shared by Whiston and Newton constitutes the main thesis of James E. Force's study. He argues, in quite convincing fashion, that Whiston, who sat at the knee (or perhaps one should say at the right hand) of the master, became Newton's lifelong and unflinching disciple. Whiston trumpeted Newton's antideist precepts, including the literal interpretation of Scripture, the belief that an active interventionist God is absolutely necessary for the orderly operation of the universe, and faith in the coming millennium, at which time the prophecies of Daniel and Saint John will manifest themselves in all their glory. But more than this, Whiston shared Newton's intense aversion—indeed, hatred—for the doctrine of the Trinity. Newton, who spent far more of his time on biblical study than on science, had scoured the Scriptures for written evidence that the Father, Son, and Holy Ghost are united in one Godhead. He found none and proclaimed this corruption of Scripture the heresy of heresies.

Whiston took the ball and ran with it, but from Newton's point of view too far and too publicly. Fearful that his standing as a government official and president of the Royal Society might be jeopardized if his religious views became known, Newton disowned Whiston after the latter began to preach and write on the subject of Arianism. When Whiston spurned official warnings to back off, thus inviting self-martyrdom, he was booted out of Cambridge. This, predictably, only served to reinforce his zeal, and the outcast went off to London to spend the rest of his life in a vain effort to convert the masses. Thus, Force claims Whiston to be "the honest Newtonian"—indeed, more honest than his master since Whiston nurtured Newton's privately held beliefs at great personal cost.

This is certainly correct as far as it goes, but it is not quite so revealing as the author would have us believe. The book began as a doctoral dissertation over a decade ago, and much of its thunder has been stolen by the recent observations of several distinguished scholars, including John C. Greene, Margaret C. Jacob, Frank Manuel, and Richard S. Westfall, not to mention Maureen Farrell, author of *William Whiston* (1981). What is more, the work, which unfortunately still reads very much like a dissertation, tells us more about Newton than about its purported subject, and this, too, is no longer unfamiliar ground. Although this book adds some depth to our understanding of early modern English intellectual life, it contributes little in the way of original insight. In sum, the definitive study of William Whiston's life and thought is yet to be written.

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NORMA LANDAU. *The Justices of the Peace, 1679–1760.* Berkeley and Los Angeles: University of California Press. 1984. Pp. xv, 421. $38.50.

In this admirable study Norma Landau argues that the period from the Restoration to the accession of George III marked a transition in English local government from "patriarchal paternalism" to "patrician paternalism." The judicial patriarch, a "monarch in miniature," was the most prominent figure by virtue of wealth, family tradition, and local knowledge in the community in which he served. The judicial patrician was a legal administrator, distanced from those he governed; he viewed his activities in a national context, as an exercise in sensible government. The transition from patriarch to patrician came about because the rise of party after the 1688 revolution meant that more gentlemen of lower status were brought on to the bench. Moreover, party politics accustomed the local justices to think of themselves as members of a collective partisan group. A complementary development was the increase in importance of petty sessions, which provided such groups with the opportunity for the regular exercise of administrative power. When, in the 1750s, party declined as an element in the selection of justices, there was no reversion to patriarchalism; men active as justices were already anticipating the attitudes of the Victorian public servant.

This challenging thesis is underpinned by a detailed examination of one county, Kent, which comes close to providing the "county community" study that has been much missed in the published historiography of eighteenth-century England. Landau gives illuminating evidence, presented with subtlety and insight, on matters as various as the composition of grand juries, the changing character of justices' handbooks from Lambarde to Burn, the political battles of the mid-1740s to secure a greater measure of Tory representation in the commissions of the peace, the law relating to writs of *certiorari*, the distinctions between urban and rural justices, and the significance of the recognizance *concordantur*, which suggests, fascinatingly, that justices of the peace had a role as local mediators.

Landau's range is so formidable that she can be forgiven the occasional slip: it was, for example, Shrewsbury, not Somers, who pressed for a purge of all the "notoriously disaffected" justices in 1696. Landau's definition of "honorary justices" is open to question: the privy councillors, judges, and law officers named ex officio for every county were clearly "honorary" and are properly discounted for purposes of numerical calculation, but it is odd to disregard the peers and peers' sons whose names were included for a particular county for local reasons. Such grandees were not often active as justices, but they were active sometimes, and to exclude them from consideration distorts the analysis of the social background from which the justices were drawn. Minor reservations apart, Landau's book is to be warmly welcomed. It is an outstanding contribution to the political and administrative history of early Hanoverian England.

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H. I. DUTTON. *The Patent System and Inventive Activity during the Industrial Revolution, 1750–1852.* Dover, N.H.: Manchester University Press. 1984. Pp. 232. $32.50.

For generations, in popular and professional imaginations alike, the Industrial Revolution has conjured up images of ingenious men devising and making machines. Yet, as H. I. Dutton reminds us, surprisingly little work has been done on the process of invention in the era when the British fashioned instruments that ultimately were to transform the world. In an effort to help rectify this deficiency, Dutton offers an intelligent assessment of one small but significant corner of the subject. The modest scope of his assessment is in part attributable to the precision with which he delineates his topic and responsibilities. This results first in a discussion of invention that carefully avoids the larger and related areas of innovation (the initial use of invention) and general technological change. The focus of investigation is then further narrowed through means of a sustained and specific consideration of the patent system and its effects on inventive activity. Even so, the difficulties of evidence and interpretation remain prodigious, a situation that, as Dutton himself concedes, reduces many of his conclusions to informed guesswork. It is to his credit that this does not seriously compromise the value of his contribution; indeed, perhaps the most impressive feature of his book is the way in which it is built around a series of clear, concise, and imaginative discussions of the criteria that must be met in order to achieve a sound understanding of industrial invention.

Dutton's approach is often less concerned with inventors and their creations than with the "business" of invention. His main efforts are devoted to tracing the birth of an infant "invention industry," its most prominent characteristics being treated to a thorough and illuminating analysis. These characteristics included the recognition and protection of knowledge as a form of private property, the production of invention as a marketable commodity, and the development of invention as a function of capital accumulation and the changing costs of production. It is from this perspective and within such

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a milieu that we encounter Dutton's inventor as human being and historical figure, and so it should come as no surprise that he is consistently portrayed as "economic man." This portrayal, however, does not simply flow from the particular context in which Dutton has chosen to examine inventive activity, for he is occupied with attacking what he calls the "more eclectic approach" to the question of human motivation (p. 104). Despite the apparent sophistication of his argument, it is unlikely that such economic reductionism will persuade those who regard the urge to invent as a social, cultural, and intellectual phenomenon in which the attractions of the marketplace may or may not play a crucial role. At the same time, it is excessive to claim, as the author does, that the pursuit of profit has not figured in previous analyses of the pioneering industrial inventors.

It should also be mentioned that Dutton's insistence on the exclusive sway of economic imperatives sometimes serves to undercut his own views on the importance of the patent system as a spur to inventive activity. Although he is very effective in revealing the system's contributions to the protection, marketing, and adoption of inventions, other evidence suggests that the causal relationship between patents and inventive output was tenuous indeed. In an examination of court decisions in the period after 1830, Dutton finds that steadily increasing support for the claims of patent holders in legal disputes had "little or no effect" on fluctuations in the number of patents taken out; instead, anticipation of future trends in the economy is seen as having been "the crucial factor" (p. 178). This becomes a highly significant point when elsewhere we are told, first, that the value of holding a patent depended almost entirely on the rigor of the law and its interpretation in court and, second, that the number of patents registered generally reflected the actual volume of inventions produced. That a rise in the legal value of patents failed to have an appreciable impact on patent registration in the 1830s and 1840s would thus seem to raise questions about the effectiveness of the system in influencing the level of inventive activity.

This is not to say that Dutton's topic is peripheral to the central issues of early industrial invention. Among other things, he succeeds in showing the ways in which the British state forged a favorable environment for the outpouring of inventive genius that marked the century under consideration. All those concerned with understanding the intersection of law, administration, and technology during the Industrial Revolution will want to consult this useful work.

PETER DUNKLEY
Georgetown University

NEVILLE KIRK. *The Growth of Working-Class Reformism in Mid-Victorian England.* (The Working Class in European History.) Urbana: University of Illinois Press. 1985. Pp. xiii, 369. $27.50.

If truth in advertising applied to books, Neville Kirk would be in serious trouble for attempting, in his title, to pass off a detailed piece of local history as an interpretation of a national trend. Even if he had substituted "the English cotton towns" for "England," he could still be held on weak ground, for he really deals with just three such towns to the southeast of Manchester (Stockport, Ashton, and Stalybridge) and only briefly and superficially takes account of developments elsewhere in Lancashire. Essentially, Kirk addresses himself to the key problem of how the English cotton factory towns were transformed about the middle of the nineteenth century from unstable hotbeds of social tension and likely insurrection into models of orderly prosperity and class harmony within a bourgeois capitalist framework.

On the basis of his three towns, Kirk offers a somewhat different interpretation from those recently provided by John Foster and Patrick Joyce: he rejects both the former's notion that the emergence of a "labor aristocracy" broke down the earlier solidarity and radicalism of the Lancashire working class and the latter's emphasis on the deliberate cultivation of employer paternalism as the most potent factor in this development. Instead, he points to the gradual improvement in real wages, especially after the "cotton famine" associated with the American Civil War, and to the equally hesitant development of a more formal pattern of industrial relations within which employers came increasingly to accept the existence of trade unions. Kirk's explanation is both more balanced and less adventurous than those of Foster and Joyce and errs too much in the direction of "on the one hand . . . but on the other"; his principal innovation comes in the final chapter, where he explores the social and political significance of ethnic and cultural diversity (that is, the "Irish factor") in splitting the Lancashire working class and in creating popular anti-Catholic Toryism.

Despite his caution, it is doubtful how far Kirk has managed to explain the "incorporation" of the working class into capitalist society, even in the context of mid-Victorian Lancashire. Far too many crucial aspects of this area and this period still remain unexplored. For example, what was the significance of the enormous wave of enthusiasm for producers' cooperatives that swept Lancashire in the 1850s? What were the long-term effects of the "cotton famine" on attitudes and institutions? And what was really going on in Lancashire's ill-charted hillbilly land, north and east of a line joining

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Rochdale, Bolton, and Blackburn? Until these (and similar) topics have been fully explored, it would be better if the modern army of social investigators of mid-nineteenth-century Lancashire stopped trying to build mountains of interpretation out of molehills of information.

DUNCAN BYTHELL
University of Durham

MARIE BOAS HALL. *All Scientists Now: The Royal Society in the Nineteenth Century*. New York: Cambridge University Press. 1984. Pp. xii, 261. $49.50.

Marie Boas Hall has written a history of the administration and internal politics of the Royal Society of London in the last century. Her main theme is the transition from the institution of the seventeenth and eighteenth centuries to the highly professional body that has led British science in the nineteenth and twentieth centuries. In the first half of the society's life, a large proportion of its fellows, who numbered over six hundred in 1800, were wealthy amateurs, noblemen and people of fashion, whose dues and other contributions were the main source of patronage. Only a small minority worked seriously at science. In modern times, election to the Royal Society is the highest mark of distinction to which a British scientist may aspire within the structure of scientific institutions proper.

The forces making for change gathered strength after 1820, the end of the forty-two-year presidency of Sir Joseph Banks. The movement was, of course, simply the instance in science of British reform in general and has been identified in the minds of historians with a famous book of 1830, Charles Babbage's polemical *Reflections on the Decline of Science in England*. At fault, in Babbage's view, were Davies Gilbert, president from 1827 to 1830; the absence of government support and encouragement; and philistine indifference on the part of society at large.

Mrs. Hall gives a highly revisionist account of this episode, but in the conservative interest. She sympathizes with contemporary defenders of the quality and mode of British science. The subject of the recent (I thought excellent) biography by Anthony Hyman (1982) is unrecognizable in the Babbage we have here, who is a nuisance and a scold. The effective reform of the Royal Society occurs, not in response to the goadings of ill-natured radicals but under the wise and temperate presidencies of the duke of Sussex (1830–38) and the second marquess of Northampton (1838–48). Thus did reform of science broaden down in the British pattern of gradualism under the aegis of royalty and the ancient nobility.

It may well have been so, though I do wonder about a certain insularity in tone and interpretation and was surprised at the transformation of Richard into Robert Owen. At all events, a history of presidential politics within the society completes the administrative aspect. It is complemented by a quartet of chapters on the role of the society with respect to the reward system in science, to mediation between science and government, to specialization and relations with other societies, and, finally, to systematic exploration of the earth. The author had no intention of discussing the content of British science, and she has little to say about its sociology. Her book is based on the archives of the Royal Society. It is brief and very well written.

CHARLES C. GILLISPIE
Princeton University

F. DARRELL MUNSELL. *The Unfortunate Duke: Henry Pelham, Fifth Duke of Newcastle, 1811–1864*. Columbia: University of Missouri Press. 1985. Pp. x, 334. $30.00.

F. Darrell Munsell's title is well chosen. The fifth duke of Newcastle was indeed among the most unfortunate of nineteenth-century political figures in Britain. He was unlucky both in love and in office—quite the opposite from his contemporary, Gladstone. Newcastle's wife, the only daughter of the duke of Hamilton, was unfaithful and hypochondriacal, a particularly trying combination. Newcastle's divorce from her (while he was still earl of Lincoln) was one of the most scandalous of the Victorian era, and the episode left Newcastle dispirited and morose for the remainder of his life. His hoped-for happiness with Lady Frances Waldegrave in later years was dashed when she chose another. Disappointing too were his children, who squandered money, gambled, and married badly. Newcastle became estranged from his father, the notoriously reactionary fourth duke, who left his son a largely bankrupt estate.

In spite of these personal difficulties, Newcastle was an active and promising politician. An intimate friend of Sir Robert Peel, he first held cabinet rank under Peel at the Office of Woods and Forests. Shortly afterward, he was made chief secretary for Ireland—unfortunately, during the Irish Famine of 1845. When he again entered office in 1852, it was as secretary for war and colonies in the ill-fated Aberdeen coalition that presided over the Crimean War. Newcastle bore the main responsibility for the disastrous military debacles. Perhaps his greatest success came as colonial secretary from 1859 to 1864. But, as before, he was dogged by bad luck: his final year in office was clouded by the Ashanti War in West Africa.

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Munsell tells Newcastle's unhappy story very well. He properly keeps Newcastle to the forefront of the narrative and never loses the reader in the intricacies of mid-Victorian politics. He uses numerous private papers to lend substance to Newcastle's portrait and works the evidence expertly into the narrative. Munsell juxtaposes neatly what we know of Newcastle's private life to his public career, showing the relationship between the two. For example, Newcastle's contemporaries were puzzled by his assiduity, on the one hand, and his frequent ineffectiveness as an administrator, on the other. He seemed unable to break away from the minutiae of his job, and Munsell suggests that Newcastle could best forget the problems of his life by immersing himself in detail. A leisured and dispassionate view of policy making was impossible for him.

Only two criticisms can, I think, be registered against Munsell. First, the chapters dealing with colonial issues begin to drag and Newcastle becomes less vivid; the author is clearly most at home with domestic politics, described in the earlier chapters. Second, Munsell might have analyzed more closely some of Newcastle's public actions. Were there repetitive behavior patterns that could help explain Newcastle's misfortunes? Was he merely a victim of bad luck? Perhaps the evidence here is skimpy, although one suspects that Munsell could have related Newcastle's actions more explicitly to theories of human behavior. These reservations, however, do not detract from the book as a whole. All in all, Munsell has rescued from obscurity an undeservedly neglected Victorian politician.

TRAVIS L. CROSBY
Wheaton College
Norton, Massachusetts

BRUCE L. KINZER. *The Ballot Question in Nineteenth-Century English Politics*. (Modern British History, number 9.) New York: Garland. 1982. Pp. iv, 302. $43.00.

The Ballot Act of 1872 has traditionally been looked on not only as one of the great achievements of Gladstonian Liberalism but also as one of its most natural expressions. Bruce L. Kinzer would probably accept the former proposition but not the latter. Rather, he argues, Gladstone's decision to adopt the ballot—a position he had previously opposed—was grounded in his desire to attract John Bright, the great Nonconformist politician, into his cabinet in 1868. As Kinzer concludes, "Bright gave Gladstone the ballot in December of 1868 and three and a half years later Gladstone gave it to Britain" (p. 249). The need for political support motivated Gladstone; high politics, not high principles, were at work.

Kinzer's argument is a convincing one, developed in the course of a careful analysis of the ballot question from its first emergence as a live political issue in the 1830s. Indeed, not until Gladstone's government took it up after 1868 would the question be as lively as it was in its first decade. Then, it enjoyed some real support in the constituencies and swelling majorities in the House of Commons. Thereafter, the issue languished in the 1840s, enjoyed something of a revival in the 1850s, and languished again in the 1860s. Only government sponsorship revived and carried it to the final triumph.

The problem, Kinzer contends, was that the ballot was a question that enjoyed extensive but not intensive political support. Those who wanted to change the system in one way or another favored the issue, but, in the nature of things, the ballot took second place to particular causes. Dissenters, for example, might favor the ballot, but they favored religious freedom more. The ballot therefore never became a test case between the Liberals and their leadership in Parliament and only rarely between an M.P. and his constituents. Kinzer has no difficulty in demonstrating this as far as Parliament is concerned, and it fits what we know about politics in the constituencies. Hence, the question needed to be taken up at the top if it was to succeed.

As useful as Kinzer's development of the politics of the ballot question is his discussion of the arguments. Perhaps most interesting is his treatment of the arguments against the ballot. He takes seriously and does not dismiss as mere rationalization the contentions that voting was a public trust to be exercised publicly and that secrecy was un-English and antithetical to all that was best in the national character. John Stuart Mill, he reminds us, was not immune to such arguments, which played a part in the debate until the end.

Kinzer's survey of the ballot question is a highly useful contribution to our understanding of nineteenth-century politics. Sometimes his enthusiasm for his topic and his desire to tell the whole story lead him to give us rather more of it than we need, but, all in all, the work is a solid and convincing one.

RICHARD W. DAVIS
Washington University
St. Louis, Missouri

JOAN CONNELL. *The Roman Catholic Church in England, 1780–1850: A Study in Internal Politics*. (Memoirs, number 158.) Philadelphia: American Philosophical Society. 1984. Pp. x, 218. $14.00

EDWARD NORMAN. *The English Catholic Church in the Nineteenth Century*. New York: Clarendon Press of Oxford University Press. 1984. Pp. vi, 399. $45.00

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The titles of these books promise more than the studies actually provide. Joan Connell's book decently explains its limitations in the subtitle. It is indeed "a study in internal politics," the politics of the English vicars-apostolic (who governed in lieu of bishops) from 1780 to the "restoration" of the ordinary hierarchy in 1850, with a substantial backward glance at the preceding two centuries. This was originally a doctoral dissertation under the direction of Emmet Larkin, and Connell proves herself a worthy disciple of Larkin in her emphasis on bishops and her reliance on original sources copiously (and idiosyncratically) quoted.

The internal politics of the vicars-apostolic, as Connell presents them, consisted largely of their quarrels with each other, varied occasionally by their quarrels with the laity, the clergy, the religious orders, and Rome. Connell's history of the quarrels is not an unusual presentation of the story, for she, like others, relies primarily on correspondence, and nothing generates more and lengthier correspondence than a quarrel. Connell's originality consists of isolating the inability of the vicars-apostolic to get on with each other as the most decisive factor in their inadequate responses to the challenges they faced. The flaw in the structure of English Catholicism until 1850 was the absence of a central leadership: demographics, not direction, shaped the growth of the church.

Whether the quarrels of the vicars-apostolic deserve the prominence that Connell gives them is debatable. Edward Norman takes the opposite view. In a study filled with admiration for the growth of the institutional church, he blandly attributes the quarrels "to the scale of events and the seriousness with which the various issues were regarded . . .[,] an unhappy side-effect of resolute activity rather than . . . a mere descent, by petty minds, into unnecessary minutiae of disagreement" (pp. 3, 4). But in his own way Norman is also smitten by bishops: John Milner, his rival William Poynter, and the egregious Peter Baines (whom Connell omits) are all "great" prelates; the cardinal-archbishops all get chapters, but Cardinal Newman does not.

The dust-jacket blurb describes Norman's book as "the first full-scale account of the English Catholic Church in the nineteenth century." Impressive as it is, this book is neither the first nor a full-scale account. It is a study of the achievements of the institutional church, emphasizing its hierarchical leaders and reexamining an old theme from the new perspective of one who previously studied non-Catholic Englishmen and non-English Catholics. Perhaps one should not criticize a book for not being another book, but Norman has missed an opportunity here. The new social history of the English Catholics is beginning to transform what was a well-worked (even overworked) field, but Norman eschews social history. He also deliberately slights intellectual history, avoiding "Newmania" but evading serious issues. The most severe criticism one can make of Norman's book is that it is no better than what any other historian might have written. English Catholic historiography is poised for an advance to a more methodologically sophisticated, less personalistic history. No historian with the requisite breadth of view has yet appeared to rise to the challenge.

Like the institutional triumph it celebrates, Norman's book is modestly impressive. It is extensively researched, both in printed books (though not journal articles) and in manuscript collections (although Connell trumped Norman with the Leeds archives); some of the editions of correspondence on which Norman relies have been superseded. The subject has clearly been worked up by an outsider, but this allows Norman the advantage of perspective and occasional fresh insight. Unfortunately, he is also frequently guilty of small errors and misreadings (really, one ought to take some pains when citing prospective reviewers). Nevertheless, this is a good, broad survey of the subject, if one is needed. English Catholic history is well summed up, but not advanced, by Norman's work.

JOSEF L. ALTHOLZ
University of Minnesota

ALAN HAIG. *The Victorian Clergy*. Dover, N.H.: Croom Helm. 1984. Pp. 376. $33.00.

Alan Haig has a sad story to tell. This is not a book on Victorian religion but a prosopography of a briefly promising and then impoverished and discounted semiprofession. A prosopography on this scale is a daunting assignment. Haig has combed national records, which he supplements with close studies of three dioceses and a host of individual vignettes. His style is clear but for the most part flat, and the detail is initially numbing. But the book concludes with some compelling paragraphs of synopsis.

The common impression of English clergymen, particularly those in the countryside, is of a socially static group. Haig, however, shakes up that impression. He demonstrates that the idyll of the country parson, like so many other things, was an essentially Victorian phenomenon. It was the product of administrative reforms in the 1830s that prohibited clerical nonresidence and pluralism, facilitated the creation of new parishes, and redistributed the income derived from the Church of England's endowments, which were enriched by the prosperity of mid-Victorian agriculture and supplemented by generous lay giving. These measures allowed England to be honeycombed with tolerably remunera-

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tive parishes served by resident clergy often housed in spacious residences.

Reinforced by heightened teaching about the clergy's mission, these provisions made holy orders arguably the most attractive of the professional opportunities open to earnest young men of the middling class who wanted to serve and be respected as well as rewarded in their community. Other professions, like the bar and medicine, offered fewer openings and more precarious prospects or, like the civil service, a more boring routine. The opportunities in the Established Church, in fact, proved too attractive. They drew in an unprecedentedly large number of young men, not enough to keep pace with the increase in the population at large but more than the church could reward with benefices. Curacies lengthened, particularly for those who had neither graduated from Oxford or Cambridge nor possessed the private means or connections with the landed class to buy a benefice or obtain one through patronage. Even in the middle years of the century, the prospects for many clergymen were dismal.

Opportunities worsened in the 1880s. Depression struck the agricultural base of most clerical endowments, severely reducing the income of previously comfortable benefices. Dependent on an increasingly indifferent Parliament for administrative readjustment, and in any case reluctant to face the need for radical reform, the church failed to adapt its arrangements to the depression. The laity, inured to a financially self-sufficient and handsomely housed clergymen, saw no need to help them.

Meanwhile, other professions became increasingly attractive. They raised their qualifications for entry whereas, in order to staff less attractive parishes, the Established Church not only admitted increasing numbers of ordinands who had not attended Oxford or Cambridge but also faced increasingly well-respected competition from Nonconformist ministers. The rising professions based their importance on specialized knowledge and skills, a pattern that the clergy did not entirely wish to follow. Their claims were based ultimately on the importance of religion, and it was losing its grip. The parishes that suffered most, culturally as well as economically, were the rural ones, which began to look like backwaters as English society focused increasingly on cities. Not a happy story.

PETER T. MARSH
Syracuse University

JANE LEWIS. *Women in England, 1870–1950: Sexual Divisions and Social Change*. Bloomington: Indiana University Press or Wheatsheaf, Brighton, England. 1984. Pp. xv, 240. Cloth $25,00, paper $10.95.

This is a well-researched, thought-provoking history of English women from 1870 to 1950. The first part of the book discusses working-class and middle-class women in relation to family, marriage, and motherhood; the second part describes patterns of employment and characteristics of women's work.

For the most part, the author rejects those interpretations that emphasize male oppression, women's collusion, and conscious rebellion. In Jane Lewis's view women redefined their situation in the male-dominated world in their own terms, thereby creating a women's culture by attaching a "different meaning" to institutions and the relationships women share with men. These have varied over time according to the women's class position, marital status, and relation to the labor market. The reader's task of searching for hidden meanings is complicated by the author's assertion that, "If women's motivation is often more complex than at first appears, then so the outcome of their actions has often differed from what has been expected" (p. xii). A change in one area of women's lives—for instance, increasing job opportunities—did not necessarily bring about change in others. The result was that potentially liberating developments did not occur. Working-class women, for example, ended up performing double toil—inside and outside the house—while middle-class "superwomen" escaped the gilded cage but still had responsibility for home and family. This book provides an explanation for why the liberation movement in the late 1960s and 1970s, calling for more autonomy and choice in all areas of women's lives, failed to deliver. Sexual freedom led to a rise in illegitimacy, which in turn led to an increase in the number of single-parent families and a rise in female poverty.

Lewis's complex model befits a complex subject, but the net result is a Murphy's Law thesis: if something bad can happen, it has or will. This pessimistic view enables her to deny that there has been a steady line of uninterrupted, unequivocal progress for women (a view I share) because women today are in the paradoxical position of having more freedom and opportunity than ever before and yet facing inequalities that seem as deeply entrenched as ever. Her strong interest in demography leads her to conclude that the decline in family size and the increased demand for women's labor were most important in promoting change regardless of any conscious attempt by women to secure it. Lewis's devaluation of the role and motives of individuals (especially middle class) and organizations that achieved many of the advances is later contradicted when she claims that the effect of the feminist movement of the 1970s on consciousness raising was similar to the effects of the world wars in changing women's expectations. Her downbeat interpretation detracts from an otherwise brilliant marshaling of

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primary sources and secondary works and a lively synthesis of autobiographical and demographic matter.

Some of Lewis's evidence is taken out of context. Mary MacArthur may once have thought that there was something to be said for the old ideal that a woman's place was in the home, but she certainly spent little time there herself. This middle-class labor leader devoted her life to organizing working-class women, who loved her even though she lived in the West End. The famous Bryant and May strike was in 1888, not 1883. It is also debatable that the predominant concern of proponents of protective legislation, particularly in the nineteenth century, was morality. Nevertheless, the book uses a greater variety of facts, challenges more assumptions, and addresses more arguments on the subject than any I know. It is especially good at fending off the perennial cry of "What about the Midlands?" It is a "must" for scholars, even though some of its strengths are its weaknesses and its labyrinthian analysis and penchant for paradox may make it too subtle for students, who might find Olive Banks's *Faces of Feminism* (1981) more readable. A prologue and a chapter on the period from 1950 to the present would have helped put some of the evidence in better perspective.

NORBERT C. SOLDON
West Chester University

JOHN GRIGG. *Lloyd George: From Peace to War, 1912–1916*. Berkeley and Los Angeles: University of California Press. 1985. Pp. 527. $30.00.

Several years before he died, David Lloyd George told some fellow Liberals who were celebrating his fiftieth year in the House of Commons, "We are at the mercy of our biographers." The Welshman perhaps feared for the worst. Some of his contemporaries, especially some military leaders and Asquith Liberals, were convinced that he was little more than a conniving, power-hungry mountebank with no fixed principles. Following his death in 1945, two people who knew him intimately—his oldest son, Richard Lloyd George, and his chief private secretary for some twenty years, A. J. Sylvester—wrote books that often pictured him in an unfavorable light. But modern scholarship on Lloyd George, and it has been impressive in both quality and quantity, has usually been quite sympathetic in tone. Many excellent books based on new materials now provide detailed analysis of various aspects of Lloyd George's life: his Welsh origins (Kenneth O. Morgan), his views on foreign affairs, 1890–1916 (Michael G. Fry), his political role during the first nine months of the war (Cameron Hazlehurst), his relations with labor (C. J. Wrigley), and his work with munitions (R. J. Q. Adams). In the third volume of his full-length biography of the statesman, John Grigg modestly claims that his purpose is not to "supplant" previous books on Lloyd George but to "supplement" them. But supplant them he has in at least one respect. No work succeeds so admirably in providing a comprehensive treatment of the whole man and his extraordinary achievements. Grigg's biography comes as close as is likely to describing the real Lloyd George. The subject matter is often familiar, but the interpretation is original and insightful.

The Asquith–Lloyd George relationship and a comparison of those statemen's strengths and weaknesses are a thread running throughout this volume. Asquith almost always suffers by comparison. Even during what must have been the most discreditable episode of Lloyd George's political career—the Marconi affair in 1912—Asquith does not emerge unblemished. Lloyd George is shown to be a generally loyal and helpful colleague during the 1915 crisis that brought an end to the Liberal ministry. Certainly he is not found by Grigg to be involved in any cabal with Balfour and Churchill against Asquith. Moreover, when Asquith is finally forced to relinquish the reins of power, he, much more than Lloyd George, is blamed for the demise of the Liberal party. Asquith's obstinate refusal to join Lloyd George's ministry, according to Grigg, "allowed the official Liberal Party to become, in effect, an anti-Lloyd George faction," thus causing irreparable harm to British Liberalism (p. 473). Grigg believes that it was unfortunate for Britain's war fortunes that Lloyd George did not gain direction of the war effort sooner. Without Lloyd George's leadership, he asserts, it was unlikely that Britain would have won the war.

Some will no doubt argue that Grigg has overstated the case for Lloyd George. But these same critics will almost certainly admit that this study does not constitute a whitewash. Despite the author's obvious fondness for his subject, no attempt is made to mask from the reader the unsavory aspects of Lloyd George's character and actions.

This masterful and illuminating volume is highly recommended to both the scholar and general reader. It leaves one eagerly anticipating the forthcoming volumes on perhaps the most important British statesman of this century.

DAVID R. WOODWARD
Marshall University

STEPHEN CONSTANTINE. *The Making of British Colonial Development Policy, 1914–1940*. Totowa, N.J.: Frank Cass, distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. xii, 326. $32.50.

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This book is a careful, archivally based survey of the historical, political, administrative, and ideological background from which emerged the Colonial Development Act of 1929 and the Colonial Development and Welfare Act of 1940. Stephen Constantine shows that, although the former measure was the first British attempt to make general provision for assisting economic development in the colonies, it was rooted in some of the specific measures that were meant to promote East African development in the 1920s. As for the latter measure, which offered a wider range of help, Constantine confirms earlier opinion that it represented a change of heart, not only among officials but also among politicians.

The narrative begins with a survey of colonial development policy before 1914, then examines the ideas that became current during the Great War, and proceeds through three chapters on East African project aid between 1918 and 1926. Because policy toward other colonies is not discussed, these three chapters should be seen as attitudinal case studies, especially of the officials in the Treasury, chiefly concerned with avoiding outlay and maintaining control, and those in the Colonial Office, who at first wanted to provide aid only insofar as colonies might thereby be moved toward financial self-sufficiency and were in general suspicious of the Ameryite "imperial vision." Official views gradually changed, however, because of Britain's own unemployment problem, but the Colonial Office was always eager to avoid premature development, excessive financial burdens on colonial exchequers, and a transfer of initiative to London. We then move on to the act of 1929, arrangements during the 1930s, and the construction of the very different 1940 measure, where we see just how important was the role of Malcolm MacDonald. The book ends with a summary conclusion on policies and policy making.

Narrative, description, and analysis are nicely balanced, blended together so that little sign of revisionism or scholarly disputation is apparent. Nonetheless, Constantine clearly has no interest in dependency theory or in models of an exploitative imperial relationship, although he recognizes the influence that business groups, such as the Lancashire cotton manufacturers, could have on Whitehall's decisions. Nor is he very interested in overseas developments: his model is one in which colonial governors propose, for reasons that are generally not examined, but the Treasury and the Colonial Office dispose—subject to intermittent control from Britain's own politicians. Insofar as white settlers, expatriate administrators, colonial governors, and indigenous populations influenced events, the full story is somewhat more complicated than Constantine's London-centered account, which, in turn, might have looked different if he had scrutinized all the dependent empire with equal intensity. Problems were not the same everywhere, nor, as Constantine observes, did the same Whitehall officials deal with all colonies. But this is only to say that the book's scope is somewhat narrower than its title might suggest. Within the limits that Constantine has quite properly set himself, the result is a fine piece of scholarship.

IAN M. DRUMMOND
University of Toronto

PHILIP ZIEGLER. *Mountbatten*. New York: Alfred A. Knopf. 1985. Pp. 784. $24.95.

After seven hundred pages of dense analysis, Philip Ziegler brings his biography of Lord Louis Mountbatten to a close with some "personal impressions." He weighs Mountbatten's faults—vanity, naked ambition, snobbishness, and a penchant to rewrite his own history—against his virtues—generosity, loyalty, consideration, and tolerance for others. The balance, according to Ziegler, still tilts in the direction of ascribing to Mountbatten the epithet "GREAT MAN" (*sic*).

The biographer, however, finds the character of Mountbatten's greatness difficult to pinpoint. Ziegler concedes that Mountbatten was not a profound thinker: his mind preferred the analytical; his character tended toward policy execution rather than initiation."'The greatest fixer of all time'" was how Lee Kuan Yew described him (p. 701). Whether that puts Mountbatten into the category of greatness is open to question, yet Ziegler is immovable on the point and clearly overawed by his subject.

The life of Mountbatten is awesome. The front endpaper of the biography traces his royal descent: he was the great-grandson of Queen Victoria, the nephew of Tsar Nicholas II, and the cousin to Kings Edward VIII and George VI. He began his naval career as a cadet in 1913 and rose through the ranks to become supreme allied commander in Southeast Asia during World War II. A temporary assignment brought him to India in 1947 to engineer the transfer of British power. He finally fulfilled a lifelong ambition of serving, like his father, as first sea lord of the Admiralty from 1955 to 1959. His death at sea, at the hands of the IRA, was a tragic end to a dramatic life.

Ziegler maintains that he was determined to write biography, not hagiography, and his portrait contains warts and all. Yet the warts are continuously overlaid with makeup. The accusation that Mountbatten abandoned Edward VIII and switched allegiance to George VI "with indecorous haste" (p. 96) is dismissed. Lady Mountbatten's spiritual, possibly physical, involvement with Jawaharlal Nehru at a

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time of delicate Anglo-Indian negotiations in 1947 is seen from a matrimonial point of view. Suggestions that Mountbatten should have pushed his opposition to the Suez operation to the point of, as opposed to threats of, resignation are rationalized. Mountbatten's resignation, Ziegler maintains, would have had no effect on Eden's determination to pursue the anti-Egyptian operation to a military conclusion. Allegations in the 1970s concerning Mountbatten's homosexuality are derided as showing no understanding of his character.

Such proclivities are the occupational hazard of any biographer faced with a larger-than-life subject. Nonetheless, historians and others will be grateful to Ziegler for producing a valuable and readable book. As Mountbatten's official biographer, Ziegler was granted unrestricted access to the vast treasure house of Mountbatten's archive at Broadlands. Never one to deny his own historical importance, Mountbatten always retained and collected material for the use of an eventual biographer. From the early 1920s he was an inveterate diarist, recorded interviews with associates, and dictated personal reminiscences. Ziegler quotes abundantly from this material. He supplements it with correspondence and oral interviews with another two hundred individuals, researched in numerous private collections (including the Lady Mountbatten papers) and in public archives (including the Public Record Office).

This biography of Mountbatten, warts and all, will remain the standard reference book on the subject, superseding John Terraine's and Richard Hough's studies. At the same time it provides the raw materials for another portrait of an individual who was more than just a "talented playboy" (p. 116) but less than a great man.

SIDNEY ASTER
University of Toronto, Mississauga

GABRIEL GORODETSKY. *Stafford Cripps' Mission to Moscow, 1940–42*. New York: Cambridge University Press. 1984. Pp. xvi, 361. $44.50.

For many years it has been possible to sort historians of inter-Allied relations during the Second World War into two categories: those who have marveled that so much was accomplished in the way of reconciling and harnessing productively the personal vanities and perdurable national interests of the parties and those who have read the record as, essentially, a chronicle of inferior, suspicion-driven choices, which amounted to so many lost opportunities to lay the foundations for a better world than the one we have known since 1945. With this deeply researched and crisply presented study of British dealings with the Soviets during Sir Stafford Cripps's ambassadorship, Gabriel Gorodetsky locates himself unambiguously in the latter camp. Without denying Cripps's political naiveté or diminishing his irritating self-righteousness, Gorodetsky develops the case for seeing his subject's basic optimism about the possibility of better relations with Russia and his intuitive grasp of Russia's real role in the widening war as pointing in a direction that, had the British government taken it, could have transformed both the course of the armed struggle and the nature of the peace that followed. In short, the controlling argument is that Russia's great need for assistance in the last six months of 1941 created a unique if fleeting opportunity for the Churchill ministry but that the rulers in London, consistently ignoring their ambassador's insightful advice and promptings, took a line that only confirmed Soviet misgivings, ensuring that the war against Hitler would have a binary rather than unitary character and that the ensuing peace would be flawed and unstable.

So rich and readily accessible are the British records that Gorodetsky has found it possible to identify with chilling precision the exact sources of the mistaken conceptions and perverse reasonings that nullified Cripps's well-aimed diplomatic efforts. Although Churchill inevitably bears the heaviest burden, chiefly on account of his anticommunist phobia and his obsession with the Middle East, the reputations of Eden, Halifax, Beaverbrook, Cadogan, Sargent, Dill, Wavell, Attlee, and Bevin all absorb some punishing blows. In many cases their failures stem from intellectual rigidity or inertia; in others, Eden's in particular, from a debilitating combination of ambition and weakness. Whatever the source of official behavior, the net effect was to delay and dilute responses to urgent Soviet needs on the comforting calculation that the Red Army might soon collapse and that the only real guarantee of eventual victory would be America's entry into the war. Indeed, few points emerge more clearly from Gorodetsky's study than the virtually complete British dependence, psychological as well as material, on American assistance and intentions even at this fairly early stage in the war.

Without question, Gorodetsky has provided solid grounds for revising many standard accounts of this period (beginning with Churchill's) in a way that acknowledges the soundness of many of Cripps's insights and the extent to which his socialist idealism had a realistic, pragmatic side. Also without question, he has given cause to reconsider the reputations of Britain's political and military leaders where their dealings with the Soviet Union are concerned—although here it is fair to add that intense concentration on Cripps and Russia leaves open to question whether those who had to cope with the entire war-borne situation might look somewhat less

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myopic on Soviet issues if the whole range of their burdens were taken into sympathetic account.

This is, in sum, a welcome and important book. Given the significance it rightly attaches to the role suspicion played on both sides of the Anglo-Soviet relationship (among many others), it is perhaps apposite to hope that one day we may have an illuminating general study of the pathology of suspicion in international affairs and the conditions (if any) for its cure.

DONALD LAMMERS
Michigan State University

KENNETH O. MORGAN. *Labour in Power, 1945–1951.* New York: Clarendon Press of Oxford University Press. 1984. Pp. xviii, 546. $29.95.

Kenneth O. Morgan has performed an extremely valuable and difficult service. His bibliography confirms this reviewer's belief that, barring the third volume of Alan Bullock's massive biography of Ernest Bevin, there is really no professional scholarly writing on the general topic of the Attlee government. Many surveys of postwar politics cover the years 1945–51 and discuss Britain's changing social structure and such problems as the pound and the Labour party. A certain number of undistinguished memoirs (among which Attlee's own is perhaps the worst) have also been written, but no solid history. Thus, Morgan had no previous work to guide or deflect him. The organization and emphasis are his own, and he has found his way through a mass of primary material with great skill.

Morgan's thesis, with which this writer entirely agrees and indeed recorded several years ago, is that the Labour administration elected in July 1945 was the most successful and productive government that Britain has seen since the war and perhaps before it. Whatever one's individual political persuasion, the historian is bound to agree that, for once, the system operated as it should: political promises were fulfilled, the majority was adequate, the cabinet provided leadership, the party stayed in line, and Parliament churned out legislation. Yet this book is not only a history of the Attlee government. It is, as the title proclaims, about Labour in power. Morgan is a Labour party fan who, like many others, finds the politics of the party frequently to be more fascinating than the politics of Westminster. Certainly this is as true today as it was forty years ago.

Still, one must insert a few reservations. Morgan is adamant about the purely Labour origins of the National Health Service (NHS). Are they really so clear? Certainly the general practitioner service was simply taken over intact from national health insurance, and hospital regionalization, with the salaried consultants, was inherited from the wartime Emergency Hospital Service. Labour's contribution was hospital nationalization and specialists available to all. Again, in the long and generally excellent discussion of Aneurian Bevan's struggles with the doctors, the cabinet, and his party's purists, Morgan neglects to mention the minister of health's greatest concession, more important even than pay beds in NHS hospitals: the grant of £60 million to buy out the physicians' equity in their practices. Without this, the government would not have been able to prevent the establishment of new medical practices in areas that already had too many doctors.

Probably the best part of the book is on Labour's foreign policy, a topic much too frequently passed by with a nod of approval in the direction of Ernest Bevin. The weakest part is on economic problems, usually discussed simply in terms of Harry S. Truman's unexpected termination of Lend-Lease, even though this was required by law. One must say that Chancellor of the Exchequer Hugh Dalton's maintenance of the pound at $4.03 and of a bank rate at 2 percent was not a sign that Britain continued to be a world power but that it was the only European power with an undamaged, if expanded and distorted, industrial plant. In a world awash in sterling and starved for manufactured goods, the grossly overvalued pound brought the highest possible prices for British exports while import controls ensured that a strong currency did not suck in foreign goods. It worked until 1949. Here, in terms of the attention he gives it, Morgan makes a bad mistake on Stafford Cripps's devaluation. The pound was not devalued to $2.40, as he says, but to $2.80. Even this, at the time, was thought too drastic.

Still, this is a fine, much-needed book. Although by no means the final word on the subject, it will last for quite a while.

BENTLEY BRINKERHOFF GILBERT
University of Illinois, Chicago

ANDREW RIGBY. *Initiation and Initiative: An Exploration of the Life and Ideas of Dimitrije Mitrinović.* (East European Monographs, number 164.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. 217. $22.00.

Dimitrije who? So, one suspects, will many readers react on first encountering this title. And, indeed, Andrew Rigby acknowledges that, until he was approached by a surviving member of Mitrinović's circle about undertaking this study, he had himself never heard of Dimitrije Mitrinović (1887–1953). Rigby's earlier books on alternative communities and on communes in Britain are relevant to understand why he undertook a study of Mitrinović's life

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and ideas. For in editorial comments the author intimates an altruism in accord with the aim of his subject's eccentric brand of utopian socialism.

Mitrinović devoted prodigious energies to fashioning an "alternative life style," a project informed by spiritual crisis and disenchantment with what Mitrinović perceived as the institutional and moral bankruptcy of Western civilization. Mitrinović was able to impress his message on those who fell under his personal sway, yet his published pronouncements and his anecdotal utterances are enshrouded, as Rigby acknowledges, in a virtually impenetrable opacity. Even so devoted a follower as Philip Mairet confessed that Mitrinović's manner of expression amounted to "towering abstractions, metaphysical allusions and extraordinary neologisms—a style which, at its best, might achieve a kind of monstrous beauty like an elephant with wings" (p. 67).

A prepossessing figure with piercing black eyes, Mitrinović was a deeply passionate and perplexing man, spiritually drawn to an eclectic mix of causes and doctrines. These ranged from revolutionary Yugoslav nationalism to peaceable world federation, from the guild socialism of S. G. Hobson to the quirky blandishments of Major Douglas's theory of social credit, from the radical individualism of Adlerian psychology to the cosmological organicism of Eastern belief systems and mythologies. His admirers thought that he was touched by what Max Weber called charisma; his detractors suspected that he was an intellectual quack, a Serbian magus in an already crowded field of self-anointed spiritual prognosticators. Rigby sympathetically claims that Mitrinović was not merely the aspiring master of an esoteric school but also a prophetic voice whose goals were more practically social. But this assessment must be compared to the judgments of such observers as Basil Boothroyd, who said that Mitrinović "had a neurosis the size of Nelson's Column and it was called 'synthesis'" (p. 22).

Rigby provides an interesting account of the internal split in the New Britain movement (NBM), between those led by Mitrinović, who wanted spiritual and personal regeneration as the movement's aim, and others who, following the lead of Charles Purdom, saw the potential for transforming the NBM into a formally organized mass movement. The reader ponders here one of those unhistorical "what if" conjectures: what if Mitrinović had not intervened to thwart the ambitious Purdom's goals and, thus, effectively disbanded the movement? Might the NBM have veered unpredictably out of the control of its founders to become a fascistic mass movement, with Mitrinović as its presumably unwitting spiritual doyen?

In later chapters Rigby turns to Mitrinović's queer attempt to form a "third force" known as the "Woodbines Masonry"—so-called because on induction one received a packet of the cheapest English cigarettes, which were used thereafter as a symbol of recognition; the password was "Have one of mine" (p. 151)! For Mitrinović this "Senate radius" anticipated the eventual achievement of a higher "social state," in which the whole world and the individual would be organically interconnected.

Perhaps the most appropriate light in which to see Mitrinović is as a failed guru, a would-be philosopher of conduct for a world in apparent chaotic decline. But there is something undefinably silly about failed gurus. Happily, in setting out the dubious claims to posthumous celebration of the life and ideas of his subject, Rigby has preserved the commonsense wisdom of an anonymous listener who, at a lecture, jotted in the margin of his program: "I implore Mr. Mitrinović to learn a little of the psychology of the English *people*. At present, obviously, he only knows the intelligentsia" (p. 110).

GEORGE FEAVER
University of British Columbia

DAVID THOMSON. *Renaissance Paris: Architecture and Growth, 1475–1600*. Berkeley and Los Angeles: University of California Press. 1984. Pp. 214. Cloth $37.50, paper $15.95.

Reconstructing the architectural history of Renaissance Paris is a difficult task. Very few buildings remain from the period prior to the reign of Henry IV, and those that do have often been altered beyond recognition. Contemporary descriptions of Paris, while remarking on the large number of fine houses, are generally silent on the details of style, and architects' pattern books, though lavishly illustrated, bear an uncertain relation to reality. Archival research can fill only some of the lacunae. Building contracts enumerate materials without describing the projects themselves; inventories after death list the contents of rooms without describing the houses that contained them.

In view of these handicaps, one must admire David Thomson for even attempting to piece together an architectural history of sixteenth-century Paris. Lavishly illustrated and handsomely produced, his book sheds light on an important period in the city's development. It takes us back before the famous urbanism of Henry IV and shows us, for example, that the principle of uniform facades so strikingly evident in the place des Vosges and the place Dauphine was already visible in the arcaded terraces of single-bay houses that lined the pont Notre-Dame in the sixteenth century. Thomson also establishes the roots of the seventeenth-century townhouse in his discussion of aristocratic and *haut-bourgeois* building. He shows how the constraints of the medieval city—the lack of space, the narrow

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lots—worked together with evolving notions of comfort, prestige, and style to produce a kind of classic townhouse. Screened from the street by a wall broken only by an impressive entrance gate, the classic townhouse was characterized by a *corps de logis* that bisected its site and allowed the principal rooms a view onto both a rear garden and an entrance court. Stables and service areas opened off of the court, and additional pavilions, often connected by galleries, were built around the court and garden as taste and necessity dictated.

Thomson is appropriately cautious in tracing the architectural evolution of the aristocratic townhouse, whose point of conception he locates in late Gothic constructions such as the Hôtel de Sens. Remaining documents are so fragmentary that it is hard to generalize about prevailing tastes in decorative style. On the whole, it seems that French architects borrowed the motifs of the Italian Renaissance but refused to be bound by the dogmas of Vitruvian classicism. Embellishment was enjoyed for its own sake and as a mark of social distinction. It is a shame that so few traces of this energetic, idiosyncratic architecture remain.

This book contains much interesting material, although it will inevitably disappoint those who expect more than an architectural history. More careful editing would have been useful. The book rambles and is occasionally disjointed. Ecclesiastical architecture, for example, seems tucked by afterthought into the concluding chapter. In some places the book is discouragingly thin. The section on households, which quotes at length an early fifteenth-century document and then moves on to describe life in the Louvre in the 1580s, is especially weak.

BARBARA B. DIEFENDORF
Boston University

ALAIN MOLINIER. *Stagnations et croissance: Le Vivarais aux XVII^e^–XVIII^e^ siècles.* (Les Hommes et la Terre, number 17.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1985. Pp. 499. 390 fr.

This is a very solid and useful book. Under the Old Regime the province of Vivarais, the present department of Ardèche, was the northern tip of Languedoc. Located on the southeastern flank of the Massif Central, the province descended like a staircase from the heights of the Vivarais Mountains in the west, down to a central plateau crossed by tributaries, and downward again to the valley of the Rhône, which formed the eastern boundary. A wild and remote vastness half-covered with forest and scrub growth, the Vivarais was an unsurpassed refuge for wolves and bandits but an inhospitable agricultural environment for a settled population.

Alain Molinier's principal themes are general stagnation and limited growth. From the middle of the seventeenth to the middle of the nineteenth century, population increased but grain and livestock production remained virtually unchanged. To make up for the deficit in cereals, the inhabitants turned mainly to chestnuts, turnips, and potatoes, supplemented with modest imports of grain shipped down the Rhône from Burgundy.

The province's only lucky break was its relative nearness to Lyons. Pulled by the economic magnet of that great city, Vivarais developed two main exports in the eighteenth and early nineteenth centuries: raw silk and wine. With the encouragement of the estates and intendants of Vivarais and Languedoc, the production of raw silk and of silk thread expanded rapidly from the middle of the eighteenth century. The planting of mulberry trees transformed the southern part of the province, Bas Vivarais, while the development of quality wines in the north, Haut Vivarais, offered increasing revenues to the communities on the Côte du Rhône. The author traces the economic evolution of the various regions within the province with an unfailing eye for historical geography and illustrates the text with a lavish array of nearly forty maps of outstanding quality.

A lengthy section on demography complements the analysis of the economy. After the killing famines of Louis XIV's late reign, the population recovered and grew but continued to suffer from a variety of deadly infectious diseases. Mortality rates dropped first, followed by birth rates during the revolutionary and Napoleonic periods. All of this is standard fare, but well researched nonetheless. Finally, Molinier rounds out and humanizes his study with sections dealing with living conditions, deficiency diseases, crime, levels of instruction, and the like. The writing is lively and engaging, even if much of the subject matter is appalling.

The principal criticism one could make of this book is that it remains too faithful to one type of methodology and one principal category of documentation, the official governmental statistical survey. The core of the book, the economic and demographic sections, is very much in the tradition of quantitative history that J. Marczewski and J. C. Toutain established in the early 1960s. The analysis of the aggregates, both economic and demographic, illuminates what can be called the socioeconomic summit, while it leaves the base in total darkness. Hence, there is no significant analysis here of landownership, tenure systems, size and management of farms, social structure, or the financial mechanisms linking the province to Lyons. Similarly, there is a whole range of topics in demographic history that can only be studied adequately

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through family reconstitution, which the author did not undertake.

From this agenda of unfinished business one could write at least two more books. In defense of Molinier's approach, he was certainly under no obligation to cover every topic, especially for a region that is still largely *terra incognita* for historians. He has served us well with this book. Can he be persuaded to write an additional one or two?

JAMES L. GOLDSMITH
University of Oklahoma

JACQUES MAILLARD. *Le pouvoir municipal à Angers de 1657 à 1789*. In two volumes. Foreword by FRANÇOIS LEBRUN. Angers: Presses de l'Université d'Angers. 1984. Pp. 287; 347.

Jacques Maillard's *Le pouvoir municipal à Angers de 1657 à 1789* is a book that only someone French could have produced: a multivolume *thèse* for which the author has looked at every relevant document. Often such works are superb, but if done mechanistically they can be as uninspired as less thorough American monographs. Unfortunately, that is true in this case.

It is too bad, for Maillard deals with a very important topic: the municipal politics of the Old Regime. The recent treatments of the French Revolution as above all a political event have highlighted the importance of Old Regime politics, which gave future revolutionaries their political educations. Especially important were the municipal politics of town and village, for this was the only level of political activity open to the average citizen.

Maillard describes in great detail the municipal government of Angers, a *ville moyenne* typical of many provincial cities of Old Regime France. He analyzes its machinery, personnel, responsibilities, and relationships with constitutents and the royal government.

The latter is the most interesting and significiant aspect of his story. Maillard highlights the two turning points in the history of Old Regime municipalities: the Fronde, in whose aftermath many lost the right of self-government, and the Laverdy reforms of 1764–65, by which the royal government imposed uniform administrations in France's cities and towns. Both were disastrous for Angers. As punishment for the Fronde, the town was put under royal tutelage. Its mayor and councilors, previously elected by a popular assembly, were henceforth chosen by the crown. After a long and expensive struggle, Angers regained a modicum of self-determination in 1729, when it won the right to submit candidates' names to the king, who still made the final choices. The system changed again with the Laverdy reforms. They themselves were good for the town (the assembly of notables they mandated was Angers's first effective representative body in a century), but the aftermath of their failure was not. When they were abandoned in the 1770s, Angers became an appanage of the comte de Provence, who ran it with an iron hand until the revolution.

Maillard describes one important result of these events: Angers's neglect of municipal improvements. The town's notables spent so much of their time—and the city's revenues—trying to regain their independence that they neglected necessary projects, like rebuilding the docks along the river Maine, which would have helped Angers's sagging economy. But the author does not describe an even more important possible result: that the grievances arising from the long struggle to regain self-rule made Angers's elite receptive to revolution in 1789. In a similar study of the municipal government of Lille (Ph.D. diss., University of Michigan, 1983) Gail Bossenga showed that such a struggle did indeed turn local notables against the crown. But Maillard stops his story in 1789.

This is not the only instance where readers may wish that Maillard had pushed his material further. His economic and social analysis of the town's population, doubtless included in deference to *Annales* traditions, is too unsystematic to satisfy social historians, and it does not even illuminate the town's political conflicts. In the late eighteenth century, control of the municipal government passed from lawyers and officeholders to merchants, but Maillard does not discuss why this happened or what issues were at stake. Similarly perfunctory is his treatment of Angers's charitable, educational, and festive institutions. Maillard lists them all but does not show their importance to municipal politics. Did arguments over the proper organization of poor relief play a role in the conflict between merchants and officeholders? Did the spread of elementary education make artisans resent their exclusion from town government? Did public festivals reinforce loyalty and solidarity during the seventeenth century but lose this capacity in the eighteenth? As these queries suggest, Maillard's is one of those frustrating books that, despite the thoroughness with which it was researched, raises more questions than it answers.

CISSIE FAIRCHILDS
Syracuse University

MARIE-MADELEINE COMPÈRE. *Du collège au lycée, 1500–1850: Généalogie de l'enseignement secondaire français*. (Collection Archives.) Paris: Gallimard. 1985. Pp. 285. 85 fr.

This book is the latest offering in the generally excellent Archives series. Each volume in this

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pocket-sized paperback series is an anthology of primary sources edited by a specialist. The documents are meant to be linked in such a way that the reader gets a concise overview of the field, including a sense of the latest historiographical trends and a modest bibliographical orientation.

The editor of the volume under review, Marie-Madeleine Compère, is a specialist in the history of French education in the early modern period. Particularly knowledgeable about the schools run by religious orders in the seventeenth and eighteenth centuries, Compère is certainly well qualified for the job of putting together an anthology of sources concerning the French collèges and lycées from 1500 to 1850. The usefulness of a book like this one depends on the adroit selection of interesting documents and on the editor's success in placing the documents in their historical context. On both counts, unfortunately, this little book falls short of one's expectations. Few of the documents published here are genuinely interesting, and the connecting narrative is confusing.

Although Compère emphasizes the importance of the sixteenth century as the formative period when most of the collèges were established, she gives little space to this development. Following the views of her mentor, the late François de Dainville, she does not seem to appreciate the importance of the public collèges founded by all provincial cities before 1560. This shortcoming is especially puzzling because of the availability of numerous monographs, of which she cites only Gustave Carré's study of the collège at Troyes (1888) in her bibliography. Excellent studies of the municipal collèges at Vienne, Toulouse, and Auch, *inter alia*, are available and contain detailed illustrative material that could have been included here.

In tracing the history of secondary schools after the high point of the early seventeenth century, Compère observes the general decline and stagnation that lasted into the nineteenth century. She is no more successful than others have been in explaining that decline. Why 1850 was chosen as the terminal date for this survey is not clear, but this decision does have the advantage of stressing the continuity or inertia of the collèges: they were hardly affected by the political transformations of the revolutionary period. Compère estimates that there were 65,000 boys in secondary schools at the beginning of the seventeenth century, when the total population of the kingdom did not exceed 18 million, and only 45,000 in 1789, when the population had reached 25 million.

This anthology may be difficult to use in graduate seminars: the selections are too short and too oddly chosen. Nor does this book leave the reader with any sense of having understood what the French collèges were all about, which is a shame, since this subject is important for both the social and the cultural historian. The bibliography is so brief as to be pretty much useless.

GEORGE HUPPERT
University of Illinois, Chicago

MAURICE GONTARD. *L'enseignement secondaire en France de la fin de l'Ancien Régime à la loi Falloux, 1750–1850*. Aix-en-Provence: Edisud. 1984. Pp. 254. 98 fr.

This book is a basic survey written by Maurice Gontard, a scholar whose studies on nineteenth-century French primary education and normal schools are standard reference works. It begins with a chapter summarizing conditions in French secondary education at the end of the Old Regime, with special emphasis on the consequences of the closing of Jesuit schools in 1761. In succeeding chapters, one for each major regime after 1789, Gontard narrates the story to 1850. Finally, he presents an excellent short conclusion that gives his major findings and interpretations.

The book is accurate, eminently readable, and logically organized. It contains a wealth of helpful information culled from the archives and existing scholarship. The narrative is regularly relieved by forceful interpretations and good anecdotal material. And it is followed by an excellent short bibliography of "essential" works.

Gontard is particularly good on the influence of politics in education and on the conflict between the church and state for control of the schools. He also demonstrates well the underlying continuities and the disparities between official pronouncements and actual practices in the almost amazing variety of secondary schools. The year 1761, he stresses emphatically, is a major turning point because it triggered the whole question of state responsibility for education and greatly increased pressures for a modernization of the curriculum. Thus, the *révolution scolaire* preceded and contributed to the Revolution of 1789. Gontard goes on to argue that the defeat of the "Ultras" in the election of 1827 was another major turning point in that it brought a meaningful end to attempts by the clergy and Catholics to recapture control of French education and assured both that public and secular schools had a future and that there would be thenceforth two systems of education. The Falloux Law simply confirmed this.

Gontard shows how much time and energy was spent on a wide variety of attempts to respond to the challenges of the 1760s. The vicissitudes of political life consistently meant that one settlement after another was overturned. By 1850, despite a "cas-

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cade" of legislation and regulations, French secondary education was essentially what it had been at the end of the Old Regime (p. 249).

Criticisms of the book could include the following: it ends abruptly in 1850 and on an unnecessarily negative comment about the inability of the French to plan for the future; it lacks a preface or introduction and an index; it does not mention any of the important scholarship outside France done on French education; footnotes, often relatively general in nature, are kept to an absolute minimum; and, finally, the narrative sometimes gets bogged down in the minutia of education bills, ordinances, and laws without some reminder of how these details altered matters in a land where the regulations on education were many and frequently changing. But these are all relatively minor problems in a book that will be invaluable both for those seeking a basic understanding of French secondary education during the period and for scholars wishing to pursue specific points.

SANDRA HORVATH-PETERSON
Georgetown University

DORINDA OUTRAM. *Georges Cuvier: Vocation, Science, and Authority in Post-Revolutionary France*. Dover, N.H.: Manchester University Press. 1984. Pp. viii, 299. $32.50.

In this important and stimulating book Dorinda Outram sets out to correct and enlarge the historical perception of the scientific and political career of Georges Cuvier (1769–1832), a man whose achievement of scientific and political power was unparalleled among French naturalists of his day. The author's professed goals are to study "how science and the social-political world interacted" and "the process by which men of learning, among them scientists, became part of the new French governing class which emerged from the Revolutionary period" (p. 2). Underlying these concerns is a more general concern with the exchanges of power in the public realm of postrevolutionary France.

A variety of myths have inevitably surrounded Cuvier, and Outram takes pains to dispel them. She shows, for example, that Cuvier's scientific and political power did not go unchallenged in his day, that his politics were not conservative (at least not for France in the periods of the empire and the Restoration), and that his geology and antitransformist biology were not shaped by a biblical literalism. Outram's introductory chapters on Cuvier's youth and his experiences at the Academy of Stuttgart shed light on Cuvier's complex personality and the scientific ideology he developed. She then proceeds to provide a valuable analysis of how scientific and political patronage overlapped in Cuvier's day—together with important insights on the conflicts between the two. The author maintains that historians have concentrated too much on scientific institutions in this period and too little on the importance of personal power. Her discussion of the working of patronage within the confines of the Muséum d'Histoire naturelle in Paris is an excellent contribution to the broader subject.

Outram reveals many of the problems and contradictions of Cuvier's career as he sought at one and the same time to capitalize on public interest in science while keeping science from becoming too embroiled in the public arena. Perhaps inevitably, however, the complexities of the author's subject and her polemical posture in the presentation have resulted in certain imbalances in her analysis. For example, while she recognizes that the poor reception of Jean-Baptiste Lamarck's transformist views cannot be ascribed solely to Cuvier's hostility to them, she unnecessarily exaggerates Cuvier's openness in his dealing with Lamarck. She suggests that Cuvier refrained from using his power at the Institut de France to silence his older colleague, but this overlooks the fact that Lamarck ceased presenting papers at the institute before Cuvier became perpetual secretary there. Similarly, while making the very important contribution of showing that Cuvier's power was limited and subject to challenge, Outram provides the reader with too little sense of how substantial Cuvier's influence nonetheless was. A footnote provides a glimpse of A. P. de Candolle repressing some of his own views so as not to alienate Cuvier at the time Candolle was seeking election to the institute. But the reader will not gain a sufficient sense of the extent to which aspiring naturalists of the time found it necessary to pay homage to Cuvier, as for example when Pierre-André Latreille and P. S. Meyranx hastened to assure Cuvier of their allegiance to him after finding themselves in the uncomfortable position of appearing to have given scientific support to Cuvier's rival, Geoffroy Saint-Hilaire.

Outram did not intend this book to be an examination of Cuvier's ideas in and of themselves. Be that as it may, her command of Cuvier's scientific ideas is not as reliable as it should be (she misidentifies, for example, the four *embranchements* that were the centerpiece of Cuvier's classificatory system and of his rejection of the idea of unity of plan). She provides a much enlarged view of Cuvier's character, aspirations, and political acumen, but she is less successful in evaluating the extent to which the content and quality of Cuvier's scientific ideas and the power he achieved were interrelated. In short, the gap between the cognitive and the sociopolitical dimensions of Cuvier's activities as a scientist still needs some historical bridging. Nonetheless, the author has made a major contribution to under-

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standing one of the key scientific figures of the early nineteenth century. She has also, in the process, called attention to a host of important issues regarding the cultivation and the culture of science in this period. This is a stimulating book, one that is bound to inspire more good work in the area.

RICHARD W. BURKHARDT, JR.
University of Illinois

GERALD L. GEISON, editor. *Professions and the French State, 1700–1900*. Philadelphia: University of Pennsylvania Press. 1984. Pp. x, 319. $35.00.

Anglo-American scholarship has recently discovered the history of professions as a topic and has produced a spate of works on the professionalization of various fields. One aspect of this process that has largely been neglected is the relationship of professionalization to the state. The present work, a selection of five papers originally read at the Shelby Cullom Davis Center between 1978 and 1980, examines this aspect in the case of France, where it is inescapable. The title promises more than the book delivers, however; no attention is paid to professions other than engineering, medicine, and academic science, and only one essay is seriously concerned with events before 1800.

For John H. Weiss, French engineers in the first half of the nineteenth century provide a test case for explaining the stability of social patterns of stratification. He asks how the state's certified engineering elite, products of the *grandes écoles*, defended their privileged positions against challenges from civil engineers in private employ, trained at the Ecole centrale des arts et métiers, and from the subordinate engineering assistants or *conducteurs*. His answer, in brief, is that the elite's defense was based on the *baccalauréat*, with its required classical education. Weiss admits, however, after much elucidation of this screening device disguised as an examination, that even in the 1840s the majority of those entering the Ecole polytechnique had not even completed their lycée studies. In all likelihood, the more powerful barrier protecting the status of engineers was the corps structure characteristic of the French administration, not the *baccalauréat-ès-lettres* alone.

Robert Fox contributes an elegant study of the scientists in the faculties of science in the nineteenth century. After the 1830s, he shows, the age of the scientist as *notable*, with personal patronage, was over. The Ministry of Public Instruction came to control scientists increasingly, turning them into functionaries who administered examinations and lectured to dilettantes. After 1870, scientists who had chafed under these conditions demanded more freedom for research and founded learned societies. Ironically, though, when in 1885 and 1896 they got the reforms they wanted, they found that the industrialists and local politicians who now supported the faculties were merely new patrons to serve.

Toby Gelfand provides a glance at the profession of barber-surgeons in the late eighteenth century, showing in a narrowly drawn study the dependence of that premodern medical profession on the form and culture of the Old Regime state. Modern general practitioners did not evolve out of the prerevolutionary barber-surgeons thanks to the growth of medical knowledge; rather, a radical discontinuity in the history of the medical profession—a Kuhnian paradigm shift—followed the revolution in the state.

Jan Goldstein traces the history of an amorphous idea, "moral contagion," from its eighteenth-century roots into the nineteenth century. This medical theory, that convulsions or hysteria were transmitted from one person to another like the common cold, arose, she holds, from certain pattern-stories in the Old Regime; it developed, however, as a function of the professional ambitions of an emergent group of physicians whom one might style alienists or "proto-psychiatrists," who tried to legitimize their discipline by attaching it to state functions and authority. Though an engaging story, this is the weakest article here, strung on a chain of isolated and scattered quotations and events too weak to support it.

The most ambitious contribution is a comparative analysis by Matthew Ramsey of the development of the legal monopoly over the practice of medicine in France and several other countries. It might seem that the advances of medicine in the nineteenth century associated with Joseph Lister, Louis Pasteur, and Robert Koch would have led to the passage of such laws. But Ramsey shows with abundant evidence that this was simply not the case. Rather, the state and the political culture of a given country were decisive in this development. France's absolute state monopoly preceded Pasteur and Koch by two generations, while in Germany and Great Britain a "free-field" medical system remained long after those advances in medical practice. On reflection, this seems eminently reasonable since the matter of a legal monopoly is as much the history of law as the history of the medical profession.

This well-edited volume has a uniformity of organization and style, as well as a good introduction by Gerald L. Geison. It is to be hoped that a similar volume might explore the professions excluded from this one, such as the legal and judicial occupations and teaching, all of which are obviously and intimately linked to the state.

THOMAS R. OSBORNE
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CLAIRE GOLDBERG MOSES. *French Feminism in the Nineteenth Century.* (SUNY Series in European Social History.) Albany: State University of New York Press. 1984. Pp. xiii, 311.

Claire Goldberg Moses's fine political history is lucidly written and cogently argued. Its greatest strength is the sympathetic treatment of Saint Simonian and utopian socialism and of the emergence of a specifically feminist movement in France in the first half of the nineteenth century. The book's weakness is its attempt to treat nineteenth-century feminism as one continuous whole, while arguing (correctly) that there was a radical rupture between pre-1848 feminism and Third Republic feminism. This puts the structure of the book at odds with its argument, and Third Republic feminism is consequently slighted.

This is not to denigrate Moses's accomplishment. Her chapters on the earlier movement, which take up nearly two-thirds of the book, are excellent. The treatment of the utopian socialists in relation to the development of feminism will undoubtedly stand as the definitive analysis for a good time to come. Moses shows real sympathy for women and for feminists of both sexes but does justice to all involved.

Moreover, Moses justifies treating feminism of the first half of the century as a whole, even though feminism did not continue as a coherent movement after Saint Simonianism disintegrated in 1834. She acknowledges that the feminist movement was subsequently characterized by "sporadic attempts" and that "feminists were clearly flaying about" (pp. 89–90), but she establishes that feminist activism of 1848 owed a good deal to the earlier utopian movements. Using this perspective, she produces a superb chapter on feminist activism during and after the Revolution of 1848.

Although Moses successfully demonstrates continuity in feminism from the Saint Simonian period through the Second Republic, she does not attempt to establish a similar continuity from the Second to the Third Republic. On the contrary, she argues that repression following the failure of the revolution and Louis Napoleon's *coup d'état* destroyed "the most advanced and the most experienced of all western feminist movements" (p. 149). Although the English feminist movement might vie for this honor, Moses has provided a fine and long overdue analysis of French feminism in the first half of the nineteenth century.

The tension between structure and content, however, remains. A chapter on the debates of the Second Empire, though excellent, does not—indeed, cannot by virture of Moses's argument—presume to show a continuing feminist movement. This leaves the discussion of Third Republic feminism as something of an afterthought, in which many important problems are oversimplified and dealt with more on the basis of secondary works than of primary sources.

Moses uses the radical rupture in mid-century to explain the weakness of subsequent French feminism in comparison with American and English feminism. This is an interesting point that we will now all have to address, but is the rupture so much greater in France than in America and England? Surely, we must concede a new beginning for English feminism after the publication of J. S. Mill's *The Subjection of Women* (1869) and accept Aileen S. Kraditor's argument that "American feminism was just beginning in 1869" (*The Ideas of the Woman Suffrage Movement* [1971], p. 201). Moreover, there may be other reasons for the weakness of French feminism. Steven C. Hause and Anne R. Kenney (*American Historical Review* 86 [1981]: 781–806) and I (*Historical Reflections* 3 [1976]: 3–24) have argued that the French feminists' republicanism and anxiety for the maintenance of the republic was a major source of their weakness. Moses contends, on the contrary, that "this alliance with the Republicans in power worked to the feminists' advantage" (p. 233), but she does not relate her argument to ours. Thus, for the moment the debate is open.

Whatever the conclusion of this debate, Moses's book stands as a fine work and is superb on the earlier period. Moses has given us for France what Barbara Taylor has done for England (*Eve and the New Jerusalem* [1983]). For the later period, readers may wish to supplement Moses with Patrick Kay Bidelman's *Pariahs Stand Up* (1982), my own *Sisters or Citizens?* (1982), and especially Hause and Kenney's *Women's Suffrage and Social Politics in the French Third Republic* (1984).

C. SOWERWINE
University of Melbourne

MARY LYNN STEWART-MCDOUGALL. *The Artisan Republic: Revolution, Reaction, and Resistance in Lyon, 1848–1851.* Montreal: McGill-Queen's University Press or Alan Sutton, Gloucester, England. 1984. Pp. xix, 211. $30.00.

Until the publication of this book, the political history of the Second Republic in Lyon was limited to François Dutacq's fine study, articles on specialized aspects of the republic, and summary discussions in surveys. Lyon's importance to the national history of the Revolution of 1848 and the city's singularly strong, successful radical movement have long merited more extended, comprehensive treatment. In this brief yet pithy account Mary Lynn Stewart-McDougall fills the gap in a way that may be definitive. Benefiting from earlier, more selective

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forays into a veritable thicket of archival underbrush, with both an eye for detail and fine historical insight, Stewart-McDougall has written the best political history of this period and city. What distinguishes the book is her treatment of politics as a complex of innuendo, accident, and personality as well as of social forces and ideological and electoral mobilization. Yet she clearly assesses dominant tendencies, in a style that is at once vigorous, authoritative, economical, and delightful.

Stewart-McDougall argues that democratic and socialist radicalism in Lyon was a moderating force in the initially tolerant but increasingly repressive political environment of the Second Republic. This goes far to explain why Lyon's workers and radicals did not rebel in June 1848. Moderation, which reflected a long tradition of secret societies and other republican associations under the July Monarchy, led to a thriving movement after the June Days and subsequent victory for the radical slate in the assembly elections of May 1849. Stewart-McDougall also underlines, as Edward Berenson has done in a different context, the creativity of the Democratic Socialists in resisting oppression—first learning from their electoral mistakes in 1848 and then availing themselves of loopholes in repressive legislation and of various organizational forms to maintain their movement despite harassment. She insists that repression actually strengthened radicalism, at least until June 1849, in a kind of dialectic "between repression and resistance, government and governed" (p. 154), which includes the interaction between the contending extremes of the political spectrum in elections. Ironically, success by either party repeatedly generated either apathy or hubris, which practically guaranteed a reversal for that party in later elections. The sequence of repression and resistance and the intoxication with electoral victory both help explain Lyon's distinctive revolt of the Croix-Rousse in June 1849. But the behavior of certain individuals and of the proletarian, paramilitary *Voraces* also had a role. In the siege following insurrection, this dialectical process effectively came to a halt, because repression no longer stimulated resistance but killed it.

This book gives special attention to the impact of Lyon's artisan workers in the formation of a strong local party. Workers were more prominent in Lyon's radical movement and legislative delegation than elsewhere, which Stewart-McDougall attributes to the existence of highly politicized, worker-controlled suburbs. She also emphasizes the absence of paternalist mediation between silk weavers and their merchant employers, an absence that intensified weavers' radical commitment. This last argument should have been made less from a consideration of the industrial or class structure of Lyon, which was singularly complex, than from an ideological and attitudinal basis. Lyon's weavers and merchants were highly interdependent by technical and organization necessity. This interdependence materialized in various channels of de facto paternalist intervention, but such channels were not commonly articulated through a paternalist ethic. Rather, they had the character of necessity and contract, engendering at once the feeling of autonomy and resentment among weavers. What requires explanation is why weavers' attitudes changed so much while their industry changed so little. The answer lies at least as much in changes of consciousness as in economic or class structure.

This is not primarily a book about consciousness, and, despite an excellent introductory chapter on the social foundations of radicalism, the analysis of structures is cursory. Nevertheless, *The Artisan Republic* has rightful claim to the authoritative status, held until now almost exclusively by Dutacq, as the best study of Lyon's unique radical experience.

GEORGE J. SHERIDAN, JR.
University of Oregon

RICHARD TERDIMAN. *Discourse/Counter-Discourse: The Theory and Practice of Symbolic Resistance in Nineteenth-Century France*. Ithaca: Cornell University Press. 1985. Pp. 362. $29.95.

Recent developments in literary theory are of little interest to most historians. The theorists' abstruse abstraction, no doubt, dismays even those in our profession still using historically significant works of imagination. But this abandonment of criticism by our colleagues in literature has produced some important new insights into the nature of language for the historical as well as the literary discipline. The theoretical concerns of structuralism, semiotics, and formalism intersect with those of intellectual and social history, as the work by Hayden White (1973) and William Sewell (1980), among others, well illustrates. Consequently, Richard Terdiman's attempt to characterize the historical features of literary discourse promises to engage more than just a few adepts of Derridean arcanum.

Terdiman elucidates the dialectical interaction of the dominant and subversive languages of nineteenth-century France. While newspapers, advertisements, and political speeches expressed a middle-class hegemony, the achievements of Balzac, Daumier, Flaubert, Marx, and Mallarmé all worked, at the level of the sign and its referent, to counter this cultural dominance. The resulting conflict did not appear in a historical vacuum but was closely tied to the development of industrial capitalism. Terdiman thus complements the historian's interest in the overt clash of ideologies in the period by examining the less apparent dialectical implications

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of the linguistic forms used in creative activity over time. However conscious their opposition to the prevailing system of signs, counter-discursive strategies never freed themselves from it: the forthright contestation in the *roman d'initiation* is enveloped by the bourgeois discourse developing in the first half of the century, just as the satirical poses adopted by Daumier, Flaubert, and Marx are subsumed by a more secure language after 1848. Even the symbolist prose poem's outright rejection of the most pervasive mode of expression—what Terdiman calls "de/citation"—remained within the linguistic space defined by the dominant discourse toward the end of the century—the situation of uncanny texts to this day.

Clearly, problems of abstraction remain: the book's argument frequently runs ahead of its evidence. At one point Terdiman even suggests, albeit playfully, that the historical significance of the forty-ninth item in Baudelaire's collection of prose poems is derived from its "oblique numerological warning of an inevitable movement to follow 1848" (p. 316). At other points the author's assertions become "laws" (pp. 61, 65), and his dependence on an uneven collection of secondary sources results in a number of factual errors, miscitations, and quotations out of context (pp. 129, 146, 322). Moreover, the book presents "culture as a field of struggle" (p. 25) while virtually excluding all functional features of languages; the dialectic involving the linguistic forms of a select literary and artistic canon thereby represents the whole of French culture. Terdiman would do well to read more works by social historians sharing similar interests in language, such as François Furet (1978) and Robert Nye (1984).

Nevertheless, this book provides a potential model that historians may usefully apply to other, nonliterary discourses in the past. By adding an important historical dimension to literary theory, Terdiman has made this theory more useful to our profession, for which we should all be thankful.

JAMES SMITH ALLEN
Phillips University

FRANCIS BRUMONT. *Campo y campesinos de Castilla la Vieja en tiempos de Felipe II*. Translated from French by JUAN MIGUEL FIGUEROA PÉREZ. (Historia.) Madrid: Siglo Veintiuno de España. 1984. Pp. xiii, 362. 1,750 ptas.

Despite its title, this work is not a study of rural life in the whole of Old Castile but rather of a small district in one of its provinces. An earlier version was published as *La Bureba à l'époque de Philippe II* (1977) in the Arno Press series Dissertations in European Economic History. This Spanish translation is considerably enlarged with the addition of an entirely new introductory section and conclusion and an alphabetical index of place names. Francis Brumont has rewritten several chapters and updated his bibliography. But the 1977 monograph remains the heart of this new volume: it is a study of La Bureba, a mountain-ringed district in the northeastern part of modern Burgos province.

The text is divided into two major sections. The first, which represents the major departure from the 1977 version, is an introduction to economic life in northern Old Castile in the second half of the sixteenth century. This section is designed to provide a broad geographical and historical setting for the second part, which is a study of the La Bureba district. The narrative is followed by an appendix of over a hundred pages of tables, graphs, and maps. This statistical section, which will be a convenient source for other researchers, includes data about population trends, land and animal ownership, crop yields, tithes, rents, artisan and service activities, meat and wine consumption, and price movements.

Brumont has based his study primarily on documents in central archives in Simancas and Madrid, but he has also used local archives and a large number of secondary works to good advantage. He is a careful scholar in the best Gallic tradition, taking great pains to examine critically the sources he is using and never failing to warn the reader when the data may be unreliable or when his conclusions should be regarded as tentative. Moreover, his prose style is lively and readable, even in translation.

The study of the La Bureba district, which in the late sixteenth century had a population of nearly twenty thousand inhabitants living in about a hundred villages, is a model of scholarly research and analysis. It contains a detailed examination—with many interesting facts and stimulating insights—of the demography, economy, and social structures of that rural area. Especially noteworthy is Brumont's treatment of property ownership, production and markets, and the question of how to define wealth and poverty in the sixteenth-century rural context.

A salient fact, not only about Old Castile as a whole but also about the La Bureba district, is the enormous diversity of conditions, making it difficult to generalize about the rural world of the time. But Brumont handles the difficulty quite gracefully. This carefully researched, meticulously documented, and well-written study should be required reading for everyone interested in early modern Spain.

DAVID E. VASSBERG
Pan American University

DON W. ALEXANDER. *Rod of Iron: French Counterinsurgency Policy in Aragon during the Peninsular War.*

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Wilmington, Del.: Scholarly Resources. 1985. Pp. xx, 260. $28.00.

The bases of this highly detailed study of French counterinsurgency operations in Aragon are the original documents at the Archives de la guerre and the Archives nationales. Rather than a study of campaigns and strategy, this is a careful analysis of the efforts of the French commanders, primarily Suchet and Reille, to subjugate Aragon, secure adequate resources for their armies, and prepare the area for annexation to France.

Don W. Alexander presents an account, unusually rich in detail, that traces French efforts to pursue and crush the *partidas*, establish fortified posts throughout the province, and secure the country in the face of monumental problems. Spanish resistance, carried on by both conventional and guerrilla forces, is examined in light of Suchet's efforts to pacify the province through the Aragonese nobles and the church and through supplemental measures such as reforms, amnesties, retribution, intimidation, and brute force. In 1812, however, after the victory of Wellington's Anglo-Portuguese army at Salamanca and Napoleon's decision to divert French reinforcements for the Russian campaign, the *partidas* gained the initiative. French forces were then outnumbered, outmaneuvered, and outfought by the guerrilla bands, which increased in size, experience, tactical ability, and confidence. The French defeat at Vitoria in 1813 sealed the fate of Iberia; Suchet and his generals struggled to retain Aragon, but they only postponed the inevitable.

Although Suchet was considered the most effective military governor in the Peninsula, Alexander's research demonstrates that Suchet failed to recognize the evolutionary transformation of weak guerrilla bands into formidable, efficient forces capable of successful operations against regular French troops; that he failed to cooperate effectively with other French generals in their common war; that he lacked conviction in the implementation of Napoleon's instructions; and that his inflexible strategy contributed to the French defeat.

This study emphasizes the cumulative nature of the war through the *petit guerres* that continued with increasing ferocity. At times, the minute information becomes almost overwhelming as details of each patrol, relief column, and operation are examined, but the reader gains an appreciation of the constant turmoil and insoluble nature of this war. Alexander's study reinforces recent scholarship regarding Napoleon's negative impact on the struggle. Clearly, imperial instructions, based on incomplete information and misconception played a major role in Suchet's failure to pacify Aragon as they did in Masséna's defeat in Portugal.

A few more details on the sieges of Lérida, Tortosa, Tarragona, and Valencia, carried on by Suchet's forces, would have been useful in understanding the overall relationship between these operations and guerrilla activities. For example, only two sentences are devoted to the siege at Tarragona, even though this was one of the most grueling sieges in the Peninsula. Alexander's suggestion that Suchet should have distributed the church lands in return for peasant support was probably not a viable option. And it would certainly have been a shock to Wellington to learn that his forces were adequately supplied.

In many ways, war in Aragon differed drastically from that in other Spanish provinces. Nevertheless, many aspects of Alexander's careful analysis and valuable insights are applicable to other Spanish provinces. The degree of intensity of Spanish resistance and the sustained response of the French might vary, but the ultimate results were similar. Consequently, this study makes an important contribution to the study of the Peninsular War. It provides details rather than generalizations, thereby increasing our understanding of the *partidas* impact on the French. Alexander gives scholars a new and original look at the daily operations in Aragon as well as a gauge by which they can measure the various levels of insurgent activity in the other provinces.

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PABLO MARTÍN ACEÑA. *La cantidad de dinero en España, 1900–1935*. (Estudios de Historia Económica, number 12.) Madrid: Servicio de Estudio, Banco de España. 1985. Pp. 115.

Pablo Martín's book, sponsored by the Bank of Spain, draws together the main Spanish monetary trends from 1900 to 1935. Despite having written only fifty-two pages of text, Martín manages to incorporate into his work eleven tables and five graphs, some of which utilize highly sophisticated logarithmic functions. The book's appendixes include sixteen additional tables.

Martín bases his theory on Milton Friedman's idea that the quantity of money affects economic development and that the distinctions between three types of money—money 1 (M 1), money 2 (M 2), and money 3 (M 3)—are relevant to ordinary people. M 1 equals total currency and checking deposits; M 2 includes those assets and, in addition, the total savings accounts and small certificates of deposit; M 3 includes large certificates of deposit. All three types of money are liquid. Eight of the forty citations in Martín's bibliography are in English, all from Friedman's Chicago school.

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By any definition of money, the circulation in Spain in 1900, when the total M 1 was practically the same as M 3, was about 3.2 billion pesetas. Over the years the gaps between M 1, M 2, and M 3 increased. Total monetary circulation grew steeply from 1915 to 1920, showing the effects of World War I. From 1920 to 1927 money growth was on a plateau, but the worldwide depression, although less severe in underdeveloped Spain than in industrialized countries, began earlier—in 1927 when a new inflation began at a slower rate. The number of bills printed dipped slightly in 1931, the year when the Spaniards founded the Second Republic. By 1935, the last year considered by Martín, M 3 was approximately 15.6 billion pesetas. Real Spanish savings throughout the period, however, show a slow, steady rise at a much lower rate than money created.

Martín breaks down all these general statistical trends in his many special tables by year and by type of holdings: cash and coin held by individuals and by public, banking, corporate, and insurance institutions. For example, he indicates how gold reserves decreased only during the years 1925–30 but generally increased throughout the 1900–35 period, including the years of the Second Republic, much maligned by the banking classes.

ROBERT H. WHEALEY
Ohio University

PAUL SERVAIS. *La rente constituée dans le ban de Herve au XVIII^e siècle.* (Collection Historique, series 8, number 62.) Brussels: Crédit Communal de Belgique. 1982. Pp. xxx, 390. 750 F.

Traditional credit markets are sometimes confusing to twentieth-century historians, both in the way they operated and in their relationship to larger economic processes. This exceptional book by Paul Servais deals with both issues as they relate to the Ban of Herve, four villages surrounding the town of Herve, in the northeast corner of Belgium. After an excellent introduction to the sources within which a historian can find evidence about the creation, transfer, and redemption of loans, the author considers three questions. How and why were loans made? What was the evolution of the credit market in the eighteenth century? And, finally, how did the credit market relate to—as either a cause for or consequence of—the movement of the underlying regional economy of the time?

The sections of the book that describe sources and the creation of loans deserve praise despite the fundamental aridity of the subjects they treat. Of special importance in this regard is the author's discussion of how loans were written down and recorded, which helps us understand which documents have been preserved for study today. Also, his description of the local economy and the family economy and the way each led to the creation of loans is very illuminating.

The great strength of this book lies in its ability to integrate the credit market with the rest of the economy. The Ban of Herve in the eighteenth century was at first a dairy-farming region, not far from the large cities of Liège and Aachen and close to the growing industrial town of Verviers. The dairy farms were small, and during the century the merchant-manufacturers found in the Ban of Herve a ready source of manpower. Many of the poor, underemployed residents of the Ban found work as spinners and weavers. The region had as its main products butter and cheese from its dairy farms and woolen yarn and cloth from its cottage industry. Servais links the movement of the credit markets to the evolution of the dairy product and the textile markets. The strong link he finds is to textiles, and as the Verviers textile industry grew after 1750, so the credit market expanded, both in terms of increased average loan value and lower rates of interest. He interprets this as a clear movement away from the role of credit in the seventeenth century, when credit had been used by an impoverished countryside to turn over its real estate to urban money-lenders, toward a nineteenth-century pattern, in which rural credit was used to finance the acquisition of land.

In recent years economic history has moved toward the integration of industrial with agricultural history. By using the credit markets as a means to see the rural economy as a whole, Servais has advanced our knowledge of how this integration worked.

MYRON P. GUTMANN
University of Texas

ANN HÖRSELL. *Borgare, smeder och änkor: Ekonomi och befolkning i Eskilstuna gamla stad och Fristad 1750–1850* [Burghers, Smiths, and Widows: Economy and Population in Eskilstuna Old Town and Free City, 1750–1850]. Summary in English. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 131.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International. 1983. Pp. 205.

Ann Hörsell's dissertation is the tenth monograph to come from the Uppsala University research project "The Family in Sweden's Demographic and Social Transformation after 1800." Her work is a microhistorical study of Eskilstuna that focuses on the impacts of the so-called free city (where traditional guild restrictions did not apply) from its foundings in 1771 through the mid-nineteenth century. Hörsell considers the patterns of economic

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development and organization that emerged in the seventy-five years after the creation of the free city, and she attempts to tie demographic and social changes to economic trends. Her interpretive position is "materialistic."

Hörsell divides the century covered into three periods: 1750–71, 1771–1806, and 1806–30/50. Her discussion of the first period provides a comparative frame of reference. Between 1771 and 1806 the center of economic dynamics in Eskilstuna shifted to the free city. While the old town stagnated, new shops concentrating on the production of metal and metal items developed in the free city. The mode of production based on the putting-out system, however, went unchanged. Between 1806 and 1830, both sections of the city grew. In the old town the number of merchants rose, and a small group of very wealthy "capitalists" developed. The society became polarized between these new managers and the workers. In the free city, specialization increased, the number of employees rose, and an elite of producer-entrepreneurs, or "proto-industrialists," developed. Eskilstuna did not become an industrial city in this period, but some of the foundations for the later very rapid industrialization of the city were laid.

In seeking the links between population structure and economic factors, Hörsell examines such variables as family size, marriage patterns, fertility, and death rates among merchants, artisans, officials, single women, and the remaining segments of the society. She concludes that economic trends affected demographic developments, but not always as expected. For example, marriage-age patterns and spouse selection preferences were clearly affected by the state of the town's economy. Marriages increased during the good periods, but these did not always result in population increases. It was found that journeymen often married late and then frequently chose the widows of master craftsmen—widows who gave them access to a shop. These marriages rarely produced enough offspring who reached maturity to replace the parents. Consequently, a surplus labor base did not develop in the city to support capitalist development.

Hörsell combines contemporary social history methods with the exceptional population source materials available in Sweden to produce a well-researched, interesting, and valuable book. What is regrettable, however, is the lack of any attempt to say something beyond Eskilstuna, to put her study into a larger context, and to say something more general about Sweden's modernization.

BYRON J. NORDSTROM
Gustavus Adolphus College

DAVID WARREN SABEAN. *Power in the Blood: Popular Culture and Village Discourse in Early Modern Germany.* New York: Cambridge University Press. 1984. Pp. x, 250. $29.95.

Few if any American historians know quite as much about the German peasantry of early modern times as David Warren Sabean. For years he has been working on an exhaustive study of family and kinship relations in one Württemberg village between the sixteenth and the nineteenth centuries. In the course of his research in various Württemberg archives Sabean has come across a number of fascinating stories that, though not pertinent to his main line of investigation, are simply too interesting to ignore. There were, for example, the peasants who refused to take communion because they could not bring themselves to forgive their enemies. There was the villager who claimed to have met an angel in a vineyard, the thirteen-year-old girl who announced that she was a witch, the pastor who engaged in drunken sprees that revealed as much about his neighbors as himself, and the village-wide conspiracy to hush up a clergyman's murder. Perhaps most remarkable—especially since it took place in 1796—was the case of the villagers who cruelly buried a bull alive in a ritual intended to wipe out a local epidemic of animal disease. Any historian, of course, would relish the chance to retell stories like these. But Sabean is concerned to use these cases as the starting point for a deep investigation into the nature of village life in early modern Germany. Precisely because these episodes were remarkable, even to contemporaries, they generated an unusual amount of written testimony and comment. Together they offer a rich opportunity to look into the heart of the society in which they occurred.

The picture that Sabean derives from these episodes is a grim one. He describes the villages as being riven by hostilities and lines of enmity—between the peasants and the authorities, between secular and religious leaders, and between or within families and kinship groups. Often the hostilities were misdirected: unable to fully comprehend the demands made on them by new theological trends or state policies, the peasants personalized their frustrations by identifying other members of the community as their enemies. On a general level, this picture is consonant with what we already know about premodern villages in Europe from such works as the English witchcraft studies of Keith Thomas and Alan Macfarlane. But Sabean's particular interest lies not only in identifying the lines of social conflict and cohesion but also in exploring what he calls village "discourse"—the language and imagery through which the villagers expressed their comprehension of the world in which they lived and the events in which they were involved. Sabean's reading of the data suggests that powerful yearnings for social justice and for the maintenance of com-

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munal solidarity often underlay what on the surface appeared to be irrational or antisocial actions.

Sabean's analysis of the villagers' mental world shows the influence not only of modern anthropological concepts but also, if less directly, of psychoanalytical and other attempts to understand the human subconscious. This approach may take some readers deeper than they are willing or able to go; others may find that the author's discursive and meditative style makes it hard for them to identify precisely the main points he wants to advance. But Sabean is always scrupulously fair to his readers. In a practice borrowed from the "close reading" technique in anthropology, he invariably summarizes or quotes at length the documentary evidence for each episode or situation before proceeding to his analysis. Indeed, this is so much the case that lecturers in social history may be tempted simply to plunder Sabean's book, skimming off his village stories for lively examples of "the way it was" in premodern Germany. But the book deserves much more careful study than that. Patient readers will find themselves richly rewarded for attempting to follow the author deep into the mental world of his quarrelsome and cranky but often inventive and articulate German villagers.

CHRISTOPHER R. FRIEDRICHS
University of British Columbia

BERND ROECK. *Elias Holl: Architekt einer europäischen Stadt.* Regensburg: Friedrich Pustet. 1985. Pp. 280. DM 56.

Few cities owe as much to one architect as Augsburg does to Elias Holl (1578–1646): his city hall still crowns the city; Perlach tower, Zeughaus, and Stadtmetzg still set decisive accents; Wertachbrucker Tor and Rotes Tor still guard the old town. And, with the possible exception of Erwin von Steinbach, no German architect has been as well remembered: Holl has been the subject of plays, poems, and novels, while art historians have long explored his life and works. Despite all this attention, the artist Holl remains elusive. The sources of his art, including the significance of a brief visit to Venice (1600–01), are still being debated, as is the extent of his dependence on designs by others, especially the painters Joseph Heintz and Matthias Kager.

To this ongoing discussion the historian Bernd Roeck has made a significant contribution. By placing Holl's achievement against the background of Augsburg's economic development, Roeck lets us understand the conditions that made it possible. In the sixteenth century the city's population had almost doubled, growing to more than forty thousand, while the situation of its craftsmen had deteriorated. At the same time Augsburg had become enormously wealthy. Roeck makes a convincing case that the ambitious building program carried out from 1588 to 1632 not only served as a self-representation that would celebrate the city's wealth and power but also was meant to provide needed work. Thought-provoking, if more questionable, is his suggestion that there must have existed a master plan, focused on Augsburg's three great fountains.

As *Stadtwerkmeister* (director of city works), Holl was responsible for the execution of this program. Roeck examines the exact nature of these responsibilities by how their discharge was rewarded. We learn, for example, that Holl seems to have paid as much in taxes as the city's other eighty-two masons combined. His extraordinary ability both as an engineer and as a manager allowed him to join the city's economic elite. We have no reports of tensions between him and his employers. To the end a craftsman not overly concerned with architectural theory, Holl seems to have collaborated so easily with patricians and painters that attempts to determine what in a particular design really belongs to him often seem misguided. All the same his buildings suggest an unusually self-assured architectural imagination, especially his city hall with its marvelous domed twin towers, meant to give it, as Holl put it, "a heroic appearance."

It is impossible to write about Holl without touching on the Thirty Years' War, which cost Augsburg its wealth and three-fifths of its population and Holl his office. In 1630 the city fathers, forced by the emperor's Edict of Restitution, reluctantly asked the Protestant Holl to take an indefinite leave. Reinstated when the Swedes occupied the city in 1632, he was dismissed for good when the Catholics returned three years later. Holl's theatrical architecture still speaks of what perished in those years.

This carefully researched and attractively produced book does not attempt a definitive study of Holl's achievement. But it lays the foundations for such a study. Thus, it demonstrates the importance of placing the history of art in the context of history, especially of economic history.

KARSTEN HARRIES
Yale University

FRANZ BOSBACH. *Die Kosten des Westfälischen Friedenskongresses: Eine strukturgeschichtliche Untersuchung.* (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte, number 13.) Münster: Aschendorff. 1984. Pp. xvi, 285.

Compared to the importance of ending thirty years of devastating war and providing a basis for the following one hundred fifty years of relative stability in the European state system, the costs of the peacemaking process in Westphalia were minor. That is

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particularly so because the costs examined are only those directly expended on or by the diplomats assembled during the 1640s at Münster and Osnabruck. The much greater treasure expended militarily and politically to force weaker parties to concessions and stronger ones to moderation are not considered.

The sharpness of the focus is, however, one of the strengths of this book. Looking strictly at the logistics or housekeeping aspects of the diplomatic effort at Westphalia (not a word about what was being negotiated there), Franz Bosbach can luxuriate in thoroughness. He uses a truly impressive body of source material; his computations seem unassailable (if occasionally subdividing the subject matter too minutely); his analysis is pertinent and sensible. The text and footnotes make the numerous interpaginated tables immediately comprehensible, and we probably have here as good an approximation of the costs of the diplomacy as is possible to obtain. Bosbach wisely does not spend time questioning the extent to which diplomats lied about the expenses they reported, but he repeatedly warns us that complete information is lacking on many items, that classification of some expenses is questionable, and that educated guesses supplement the hard facts.

Bosbach provides many interesting generalizations, one or more for each subsection of his work and several about the overall process at Westphalia. Each is based on or supplied with numbers. The eighty-two parties represented at the peace negotiations spent 3,205,219 *Reichstaler* outfitting, coming, going, lodging, eating, drinking, keeping warm, reporting, entertaining, and, yes, negotiating. The representatives of impoverished German principalities spent much less than did the entourages of the European "powers" with their lengthy presence and numerous and highly specialized personnel. But even the lesser diplomatic fry lived very well indeed. All the ambassadors begged their principals for more money and many lived on borrowed funds, but quality meat and drink was consumed in great quantity (much of it, incidentally, obtained locally) and putting on a worthy show was considered essential.

It is the detail, however, that most intrigues. Bosbach works toward his statistics from a profusion of discrete cases. Explaining his methodology at every turn, he treats us to hundreds of examples that illuminate his points. He arrives at costs—of horses, carriages, guards, cooks, rent, fuel, paper, postage, messengers, clothes, gifts, bribes (very few), and so on—but continually explains routes, time frames, practices, functions, and all sorts of housekeeping "trivia" both in general and in detail. The almost infinite variety of arrangements and circumstances that make his generalizations difficult is amply illustrated. The text, footnotes, tables, and an excellent index acquaint us with hundreds of people great and small, the products and services they used or provided, and, of course, the prices.

HEINZ E. ELLERSIECK
California Institute of Technology

PETER HERSCHE. *Die deutschen Domkapitel im 17. und 18. Jahrhundert.* Volume 1, *Einleitung und Namenslisten*; volume 2, *Vergleichende sozialgeschichtliche Untersuchungen*; volume 3, *Tabellen.* Ursellen, Switzerland: The Author. 1984. Pp. 304; 207; 261. 72 FR.

Peter Hersche has computerized fifty thousand data on German cathedral chapters between 1600 and 1803 and subjected them to a variegated analysis. Among the rubrics are the background of individual canons, average age, education, length of tenure, reasons for termination of incumbency, capitular offices, groupings by family, geographical patterns, and chronological changes. Hersche also provides 120 tables and lists 3,675 canons by chapter and by family.

The limitations of his study are that he examines only the twenty-seven chapters of *reichsständische* bishoprics, which means that Basel, Liège, and Strasbourg are included but that Besançon, the suffragan sees of Salzburg, and the chapters of Lorraine, Austria, and Bohemia are not. Only Catholic canons come under scrutiny, which may arouse doubts about the inclusion of chapters where Catholics constituted a distinct minority. More serious is Hersche's focus on academic graduates rather than on higher study in general, which understandably would have necessitated unending research in local aristocratic archives.

Some of Hersche's findings are not surprising and confirm what has long been suspected. No one will wonder that less than 1 percent of noble canons took degrees in theology or law and that the higher a canon's social rank, the greater the likelihood of episcopal election, resignation to marry, and egregious pluralism. But many conclusions are quite unexpected. For example, Hersche explodes the notion that only younger sons were sent into the church; many older ones were as well, a practice that operated efficiently as an acceptable form of family planning. Second, although it has long been known that in their struggle to capture the north German episcopate the Wittelsbachs bested the Habsburgs until the mid-eighteenth century, Hersche demonstrates that at the lower level of the cathedral chapters Austria vigorously expanded at the expense of Bavaria. Third, theology, after flowering in the seventeenth century, again lost out to law in the eighteenth century. Fourth, that 49 percent of all bishops elected from within had never occupied any capitular dignity indicates that such officeholding

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was not indispensable. Fifth, 79 percent of all canons came from families that experienced at least one elevation in social rank during the period; the vast majority thus belonged to the upwardly mobile elements of the noble and bourgeois orders. Sixth, the illustrious Schönborn family was atypical in many respects: merely in the second class of families ranked by number of canonries (thirty-three), it placed well above the average in pluralism, dignities, and episcopal seats (twelve).

Hersche's handling of the evidence is so prudent and respectful that I find little to dispute until the final section, where he seeks to prove that social and economic reasons existed for the swelling tide of criticism of the imperial church. Specifically, he believes not only that the bourgeoisie were finally almost completely excluded but also that chapters were losing power to the bishops, becoming increasingly secular, and simultaneously less and less able to provide places for the aristocracy because of growing pluralism, longer tenure of canonries, and a rising aristocratic birthrate. In his eagerness to link the material world with the world of ideas, Hersche distorts both history and his own evidence. The effective "secularization" of the activities of the cathedral canons occurred in the High Middle Ages, and I am not convinced that anything essential changed thereafter. As for the bourgoisie, its representation in chapters did decline, but only from 11.6 to 9 percent. (If anyone deserved to be angry, it was the imperial princes, who had been systematically driven out of many chapters and whose overall share dropped from over 5 per cent to under 1 percent. They were also in a much better position to exact revenge.) Pluralism seems to have grown after 1750, but as a continuation of another secular trend. Finally, the evidence for an aristocratic baby boom is contradictory indeed; one of the families Hersche cites (the Kesselstatt) vitiates his thesis by its successful placement of sons in chapters throughout the empire. It may be that Hersche is on to something here, but to demonstrate it he will have to return to old-fashioned research methods in local archives. In this book he has otherwise done a fine job deserving of our thanks.

LAWRENCE G. DUGGAN
University of Delaware

MARGRET KRAUL. *Das deutsche Gymnasium, 1780–1980.* (Edition Suhrkamp, number 1251, new series, number 251.) Frankfurt a/M.: Suhrkamp. 1984. Pp. 247.

German states and constitutions come and go, but the German gymnasium appears to go on forever, at least in the Federal Republic. As flexible as it is tenacious, the gymnasium has been pronounced dead almost as often as the novel and the opera. Three Reichs, two republics, and an American occupation hellbent on turning it into Wilhelm von Humboldt High notwithstanding, it nonetheless continues to dominate the lives of German adolescents (well, of some German adolescents). Not surprisingly, enrollments have tripled and quadrupled since the 1850s and even the 1950s but still include only up to 20 percent of a given teenage cohort.

Originally conceived as a Central European version of the local Latin school, the gymnasium concluded a Faustian deal in the 1780s with an enlightened Prussian bureaucracy. The result was a kind of public school *sui generis* before Rugby's Dr. Thomas Arnold was even heard from. As Margret Kraul makes clear, no *Gymnasialdirektor* thereafter needed to be told which school was the school of the nation, not to mention the school of the army. If the Battle of Waterloo was won on the playing fields of Eton, there is at least as strong a case that the big battles of German history from Sedan to Stalingrad, and a lot more besides, were won and lost in gymnasium classrooms.

As political, intellectual, and curricular fashions changed, Kraul shows, the gymnasium changed to meet them, incrementally jettisoning Greek, then Latin, then history, while taking aboard—usually reluctantly—the modern languages, modern science, physical education, music, girls, equal opportunity, and the open classroom. But for the most part it has been a temperamentally conservative, unabashedly elitist institution, devoted to meeting political expectations on the one hand and defending its hegemony in education's inevitable class wars on the other. In one way or another, nearly everyone has felt the consequences, for better or worse, in the historic successes of German science and scholarship and the historic disasters of German politics.

Not the least of Kraul's many virtues is her skill and success in finding and keeping the thread that holds together two hundred years of ministerial red tape. Her study combines clarity of overview with a shrewd sense of contemporary detail, be it Biedermeier, Wilhelmine, or Pepsi-generational. The book also performs the considerable service of matching policy with performance by testing the pronouncements and guidelines of public figures, civil servants, and school officials, mostly in Berlin, against the real-life gymnasium in Minden, a Lower Saxon Middletown.

But perhaps its greatest single virtue is the author's sense of school history as social history, which makes her both willing and able to explain how Ichabod Cranes could become Professor Unraths; how aristocratic dedication to the Hellenic good and true could become at once a pillar of, and a private alternative to, the Christian-conservative, then imperial-Darwinian, then Nazi-barbarian status

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quo; and how the middle class could nonetheless find happiness and social mobility in a system that met it halfway but rarely more.

Lovingly documented, elegantly constructed, beautifully written, and commendably concise, this is a very good book and a real contribution to the history of education.

DAVID SCHOENBAUM
University of Iowa

WOLFGANG WEBER. *Priester der Klio: Historisch-sozialwissenschaftliche Studien zur Herkunft und Karriere deutscher Historiker und zur Geschichte der Geschichtswissenschaft, 1800–1970.* (Europäische Hochschulschriften, third series, Geschichte und Ihre Hilfswissenschaften, number 216.) New York: Peter Lang. 1984. Pp. 613.

German historical writing continues to attract attention because of its important contribution to the professionalization of historical scholarship. Most commentators are struck by the dominance of historicism, the prevalence of nationalism, and the closed nature of the guild (*Historikerzunft*). Instead of accepting the apologetic explanation of superiority in scholarship or the critical charge of political functionalism, Wolfgang Weber uses an internalist history of science approach. In order to analyze socialization, selection, and tradition, he has compiled a database of 702 chairholders of history, the vaunted *Ordinarien* who controlled the historical profession in the German universities.

Weber's painstaking research provides nuanced information on the origins, training, and careers of German historians from the early decades of the nineteenth century until 1970. His discussion of religion, schooling, and first jobs produces new material, and his presentation on the second dissertation (*Habilitation*) and on the process of appointment to a full professorship is excellent. Similarly, Weber sketches in much detail the general pattern of German historical schools (with the Rankeans overwhelming the Droysen students). He is especially sensitive to the educational deficit of German Catholics, and his sections on Austria and Switzerland (less so on the German Democratic Republic) offer valuable contrasts to the dominant Protestant Prussian *Bildungsbürgertum* mold. On first glance his conclusions seem unexceptionable: "Origin, schooling, and study . . . are characterized by relatively great uniformity. . . . Study as well as career before appointment take considerably more time and . . . training concentrates more and more on the university itself" (p. 187). In the end historicism triumphed "because its founders understood how to nurture loyal followers and to place them in the most important positions of the discipline so that outsiders were excluded and isolated from the beginning" (p. 355).

On closer inspection, this book turns out to be less convincing as an intellectual argument than as a compendium (even without an index). The quantitative section suffers from myopia in the crucial issue of categorizing the social background of historians. Instead of using a sophisticated multidimensional scheme (see the article by William Hubbard and Konrad H. Jarausch in *Quantum-Information* 11 [1979]: 10–19), Weber employs only three strata that obliterate crucial distinctions and assigns doctors and lawyers to the industrial sector! Moreover, his definition of schools is too formalistic (resting on official sponsorship of the dissertation or *Habilitation*) and too extensive (pursuing descendants through five generations). For example, did Theodor Schieder think of himself as a Rankean, or does Werner Conze consider himself a partisan of Droysen? As the author himself admits, there is some circularity of reasoning in not systematically pursuing those who failed to become *Ordinarien* (in order to establish their differences) and in not seriously analyzing the intellectual content of the competing camps. Hence, this study provides essential social and institutional underpinnings for an analysis of the German tradition of history, but it does not offer a compelling explanation of its exceptionalism.

KONRAD H. JARAUSCH
University of North Carolina

VERNON L. LIDTKE. *The Alternative Culture: Socialist Labor in Imperial Germany.* New York: Oxford University Press. 1985. Pp. x, 299. $29.95.

The title of Vernon L. Lidtke's fine study states its major thesis. The socialist labor movement, within which he correctly includes the free trade unions, offered the vision and, to a considerable degree, the reality of a genuine alternative culture to the social realities of imperial Germany. Lidtke challenges the concept created by Guenther Roth and others of a social democratic "subculture" by stressing the manifold connections that existed between society and the labor movement. He likewise discards the parallel conception of negative integration that in his view underestimates the positive integration of the movement as well as its seriousness as a destabilizing force.

Lidtke takes over from M. Rainer Lepsius the concept of a sociocultural milieu of socialist labor. He stresses the diversity, complexity, and contradictions within the milieu, the eclecticism that resulted from adopting various elements of the European and German cultural traditions, and the lack of a coherent Marxist cultural theory, even on the part of the radicals. He shows that the differences among

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radicalism, Marxist orthodoxy, reformism, and revisionism, which played such a large part in the politics and ideology of the Social Democratic party, had, apart from educational programs, hardly any relevance to the cultural and recreational activity of the movement.

The book deals mainly with the various associations that catered to such specific interests of the workers as choral singing, gymnastics, and swimming, with club life, and with the festivals of the movement. Lidtke also discusses the lyrics, music, uses, and functions of songs and the identity and solidarity created by symbols, customs, and use of language. In addition, he treats the not very successful attempts of the movement to create a literature of its own as well as its schools, educational programs, and libraries.

Lidtke draws on the large amount of secondary literature and on a wide reading of printed and archival sources. The main strength of the book is that it brings a fresh approach to the subject and attempts, successfully, to analyze the underlying conditions, needs, assumptions, and attitudes that created and maintained the alternative culture as well as the forces within the movement that gradually made for a greater degree of organizational centralization and unification of content.

Lidtke deals exclusively with the culture of the socialist labor movement and not with working-class culture as such. He thus excludes such elements of the sociocultural milieu as the experience at the work place, the tradition of guilds carried on in some occupations, the forms of family life, the role of children and women, and the importance of the neighborhood. He does not discuss the relationship of the milieu to nonsocialist labor, to popular culture nor the impact made on it by such socializing agencies as elementary schools, the army, the churches, and the institutions of social insurance. While he strongly emphasizes the creation of a distinct world by the socialist labor movement as a dynamic process, he does not examine the disintegrating force exercised by the tentative beginnings of a new mass culture that was not class-oriented and manifested itself especially in mass sports, mass communication, and mass consumption.

These remarks are not meant as criticism but rather as reference to other fruitful subjects for research in the field. They should not distract from the merits of an extremely well written and original book, which in my view presents the best insight to date into the social and cultural milieu of the socialist labor movement in imperial Germany and its meaning to the movement's members.

GERHARD A. RITTER
University of Munich

MICHAEL GRÜTTNER. *Arbeitswelt an der Wasserkante: Sozialgeschichte der Hamburger Hafenarbeiter, 1886–1914.* (Kritische Studien zur Geschichtswissenschaft, number 63.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 331. DM 68.

Until the Fischer controversy of the 1960s the writing of history in Germany was dominated by the Rankean historicist tradition of a narrative and event-oriented history that concentrated on high politics and virtually ignored the "little man." By the early 1970s the pioneering role of Fischer's "Hamburg school" had been assumed by Hans-Ulrich Wehler and Jürgen Kocka of the "Bielefeld school" of historical social science, who championed a more radical social history of politics that was much indebted to a rediscovered and imported (via American social science) Max Weber. Between them, these two schools seemed at last to have interred Ranke and historicism by the late 1970s, although West German historiography still lagged far behind the French *Annales* school and the new departures initiated by E. P. Thompson and the British neo-Marxists generally. Then, quite suddenly, in the early 1980s the Bielefeld position—which, in any case, was probably never accepted by more than about a third of German university history departments—was vociferously challenged by a horde of academic and popular practitioners of what became known as *Alltagsgeschichte* (the history of everyday life). Its devotees embraced a variety of methods and interests, including oral history, local history, quantitative methods, and those of historical anthropology. In certain quarters the "new wave historiography" was denounced as romantic neohistoricism, while some foreign observers claimed it was a frantic attempt to catch up with themselves.

Michael Grüttner's social history of the Hamburg dock workers from 1886 to 1914 may be regarded as part of this catching-up process. He presents a "social history from below" and is intent on identifying and explaining the militancy and culture of the Hamburg dock workers—as opposed to the labor culture of the trade unions and the Social Democratic party—before they were organized, segmented, and tamed in the period after 1907. His primary concern is with the "structural interconnection of social situation, social movement, and collective mentality" (p. 15), but he is equally determined to integrate the methods and insights of the new German "history workshop" movement with more established Bielefeld verities. Basing his study largely on materials in the Hamburg State Archives, and especially on the newspaper and other files of the Hamburg political police, Grüttner first provides the reader with a detailed description of the living and working conditions of the dock workers. The second part of the book consists of an account

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of the class struggle on the waterfront, revealing an authentic and spontaneous working-class militancy that gradually succumbed to the organizing efforts of the trade unions on the one hand and employers on the other.

Much of what Grüttner has to say, when he is not belaboring the obvious (as he sometimes does), is less than startlingly new, for, as he readily concedes (p. 313 n. 11), a great deal of work has already been done on the labor movement, working class, and dock workers of "Red Hamburg," and some of it has anticipated Grüttner's own conclusions—without the benefit of Antonio Gramsci, Nicos Poulantzas, or E. P. Thompson. Yet he has undoubtedly mastered his subject and presented his conclusions in a fresh light with rare clarity and without going overboard politically or embracing uncritically "thick description" or any other methodological fad. He will almost certainly not be accused of romantic neohistoricism, for his attempt to give balanced and judicious application to new methods without giving gratuitous offense to Bielefeld has in this instance resulted in a measured, instructive, and up-to-date social history that meets the highest international standards. As such, it is typical of the excellent work now coming out of Hamburg.

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ARDEN BUCHOLZ. *Hans Delbrück and the German Military Establishment: War Images in Conflict.* Iowa City: University of Iowa Press. 1985. Pp. xiv, 191. $20.00.

Hans Delbrück, whose *History of the Art of War* is one of the great historical classics (its translation into English is still not completed, even though the first volume was published in 1900), was one of the founders of modern military history. Following the example set by Wilhelm Rüstow, he insisted that warfare had to be examined historically, which in turn meant that its social, economic, and political dimensions had to be taken fully into consideration.

Such ideas were anathema both to the Prussian General Staff and to the professoriate (who, Teutonically, lack their final *e* in this book). The officers were so obsessed with the idea of the "battle of annihilation" that they looked for such a strategy even where none existed. One of Delbrück's first tangles with the General Staff was with Adalbert von Taysen and Colmar von der Goltz, both of whom insisted, with blissful disregard for the historical record, that Frederick the Great had been an advocate of the strategy of annihilation. Delbrück became Alfred von Schlieffen's great opponent, constantly arguing that decisive battles of annihilation were virtually impossible in modern warfare and warning that strategic problems could not be solved with immutable formulas and single, simple solutions. Ironically, Schlieffen's obsession with the Battle of Cannae, which he saw as the perfect example of the battle of annihilation, began when he read Delbrück's account of the battle. None of Delbrück's criticisms worried Schlieffen and his school, for how could a mere civilian understand the intricacies of military affairs?

The professors of Berlin University were equally scornful of Delbrück's attempts to write military history. To them it was not a fit subject to be studied at the university, for it was a mere *Fachwissenschaft*—a practical adjunct to a trade, a useful tool for soldiers, but not an academic discipline. His disputes with his academic colleagues provoked extraordinary outbursts of silliness. When he pointed out that Herodotus's account of the Battle of Marathon was clearly exaggerated and that ten thousand heavily armed men could never have run 4,800 feet in perfect formation and then have fought a decisive battle, Ulrich von Wilamowitz-Moellendorff suggested in the *Historische Zeitschrift* that there was no reason to doubt the intervention of the gods. Berlin University refused to appoint Delbrück professor of military history, but in 1896 he was made professor of world history, a topic on which he dutifully published a rather uninspiring five-volume work.

As editor of the *Preussische Jahrbücher*, Delbrück was an important political voice. Just as in his military history he tried to bridge the two worlds of the army and the university, so in politics he equally unsuccessfully tried to find a middle road. He discounted the "Red menace," which was an integral part of conservative mythology, and realized early on that the real danger came from the radicalized Right. He was a moderate on the question of war aims, but that a "moderate" could demand a German India and substantial territorial gains in Eastern Europe was an indication of the madness of the times. He voiced his strong misgivings about unrestricted submarine warfare and believed that the High Command's idea of "peace through victory" was a dangerous illusion. His attitude toward the Weimar Republic was similar to that of Max Weber or Friedrich Meinecke—he became a "republican by reason," lacking any emotional commitment to the new state. Nevertheless, he was sharply critical of Ludendorff's testimony at the Reichstag hearings on the causes of Germany's defeat, and he was the most convincing critic of his strategy and his attempts to justify it in *War Strategy and Politics*. Although he spoke out courageously against the "stab in the back" theory and consistently criticized the official histories of the war, he was also an impassioned opponent of the war guilt clause of the Treaty of Versailles.

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Arden Bucholz's excellently written and enjoyable book is an admirable introduction to Delbrück's life and work. Although concentrating on Delbrück's work as a military historian and his relationship with the General Staff, this study illuminates many other aspects of the Germany of his time. Delbrück's fate was to be a liberally minded man in a country where liberalism had not taken firm root and at a time when the liberal world view was shattered. He died in 1929, a year after President Hindenburg and prominent figures from business, the military, and the university had feted him as Germany's greatest historian, the man who had courageously spoken out against the guilt clause of the Treaty of Versailles. Perhaps he was fortunate to die when he did and was spared the fate of becoming further disillusioned or being similarly coopted by the Third Reich.

MARTIN KITCHEN
Simon Fraser University

ROBERT WELDON WHALEN. *Bitter Wounds: German Victims of the Great War, 1914–1939*. Ithaca: Cornell University Press. 1984. Pp. 245. $24.95.

This is a welcome monograph for students of modern social history, especially for those interested in the German interwar cohorts. The book is chiefly about the wretched victims of the Great War. Robert Weldon Whalen first takes the reader through the perils of frontline duty during World War I and conjures up the horror of the global conflict for the German side. The figures he provides about the dead and wounded are numbing. He then remains with the wounded veterans of the war, as well as with the widows and orphans of the fallen men, through the years of the Weimar Republic.

What unfolds is a story of misery and deprivation, of state neglect and lack of human concern for the deplorable fate of those who had been hailed as heroes when they enlisted in 1914 and served after 1918 only to annoy the sensibilties of well-to-do citizens. Despite the National Socialist propaganda, which before and after 1933 attempted to fuse the legacy of World War I soldiers with Hitler's own service record and, thereafter, with his record of political success, the disabled veterans of the Great War remained pariahs in the Third Reich. This, then, is the third major lesson, arising from the final scenario in the book: the crippled, forgotten warriors were inexorably doomed in Hitler's martial system, a system good only for the survival of the fittest. As a matter of fact, from other recently published accounts it is clear that some of these cripples were even included in the forced "euthanasia" program involving feeble asylum inmates on the threshold of World War II.

Although Whalen imbeds the story in a largely conventional historical narrative, he turns commendably to areas hitherto neglected in the social history of modern Germany. For example, there are interesting details about the sorts of disabilities inflicted on the warriors and the kinds of diseases suffered by the soldiers before and after the war's end. A self-contained chapter deals with "medical solutions" to those problems. In the realm of psychology, Whalen devotes much attention to the mental anguish endured by these hapless veterans and their closest relatives, by widows, and by orphans. Those people had no reason to look back on the war as a heroic and uplifting experience, the way so many nationalist writers misrepresented the character of the conflict to millions of German readers in the 1920s.

Whalen makes effective use of statistics in the book, even though the quantitative historian will find some of the tables deficient in format and sometimes lacking in essential information. But, on the whole, this social scientific approach enables Whalen to make enlightening differentiations, such as those between upper, middle, and lower classes, and to make feasible the occasional, and much-welcomed, comparison of the German situation with that in Allied countries, notably, England and France. Carefully selected illustrations by George Grosz, Käthe Kollwitz, and Otto Dix enhance the volume, as do the realistic photographs.

Indubitably, the study is valuable overall as a further contribution to understanding the degree of continuity from World War I through the Weimar Republic and, indeed, to the Third Reich. The onuses placed on the republic by the war hampered its progress toward a state of enlightenment and social justice. To a large part the initial success of the Nazis was predicated on this failure. The case of the German war victims serves to illuminate the tragic links of causality in Germany's history, from 1914 well into the second global war.

MICHAEL H. KATER
York University

EVE ROSENHAFT. *Beating the Fascists? The German Communists and Political Violence, 1929–1933*. New York: Cambridge University Press. 1983. Pp. xvi, 273. $39.50.

Two factors invariably included in discussions of the downfall of the Weimar Republic are the dogmatic policies of the Communist party (KPD) and the debilitating escalation of political violence. Eve Rosenhaft's original study of the KPD in Berlin during the republic's final years throws interesting new light on both problems.

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The first half of the book deals with the formulation and downward filtration of Communist policy from Comintern to streetfighter. As a revolutionary party, the KPD did not shrink from the use of force. At the same time the party wanted to control political violence to prevent it from becoming either an end in itself or a pretext for suppression by the state. Communist policy in the early 1930s was directed against both the Social Democratic party (SPD) and the growing Nazi threat: through the creation of a "united front from below," forged by strikes and mass demonstrations, the KPD hoped to detach workers from the SPD, whose leaders were reviled as "social fascists"; against the National Socialist party the KPD conducted a "*wehrhafter Kampf*," which encouraged "mass terror" while condemning acts of "individual terror," that is, violence perpetrated by individuals or small groups.

While the economic misery of the depression created new opportunities for the KPD, it simultaneously undercut the social bases on which party policy rested. The main dilemma was caused by unemployment: on the one hand, it provided a powerful stimulus for mass mobilization; on the other hand, its impact on the party's rank and file rendered impossible the tactics envisioned by its leaders. Because its members tended to come from the less skilled and younger elements of the working class, those last to be hired and first to be fired, unemployment had a disproportionately severe—and politically debilitating—impact on the KPD. First, it produced an economically vulnerable and volatile membership that was incapable of sustained action, particularly economic strikes and boycotts. Second, it shifted the locus of working-class experience and activity away from the factory to the neighborhood, causing theoretical and practical problems that the KPD's leaders were unable to resolve.

Marxist-Leninist theory proceeded from the assumption that the workplace was the crucible of working-class experience and political activity. Massive unemployment invalidated this, making labor exchanges, welfare offices, and residential neighborhoods the primary focuses of Communist working-class experience. From the point of view of the party this was not entirely undesirable, since it added new points of friction to those already existing between members of the lower classes and the state. Rosenhaft shows how not only police activities but also the more benign interventions of educational and welfare administrators were perceived as hostile and oppressive by many elements of the Berlin working class and how, in SPD-dominated Prussia, this may have generated some popular support for the KPD's absurd "social fascist" line. Unemployment and the exploitation of neighborhood-related issues such as tenant strikes helped win new adherents to the KPD, but the shift of locus from factory to neighborhood weakened the party's control over the selection of issues and the means used to contest them, reducing opportunities for "mass terror" while enhancing those for unsanctioned "individual terror." This was demonstrated in the *wehrhafter Kampf* against invasions of former bastions of communism by Nazi *Sturmabteilungen* (SA). Rosenhaft graphically depicts the manner in which these incursions, primarily in the form of the establishment of SA taverns, were made and the effect they had on working-class neighborhoods. By first trying to elevate what were essentially struggles for turf into actions of mass terror and then rejecting them as acts of individual terror, the KPD's leadership confused and alienated many of the rank and file, who perceived the spread of SA activity in working-class districts in raw existential terms and continued to counter SA violence with violence of their own.

In the final chapters Rosenhaft shifts her focus from the interplay of KPD policy and social environment to the campaign against Nazi taverns, the shape of neighborhood violence, and a profile of Communist streetfighters. Although not entirely conclusive, her findings suggest, as do the recent studies of the SA by Conan Fischer and Richard Bessel, that the political violence of Weimar was often generated and shaped more by specific demographic, geographic, and socioeconomic factors than by ideological considerations.

Rosenhaft has constructed a unique portrait of "communism with a human face" that illuminates a neglected area of Weimar history. Although it remains true that KPD policies were generally unrealistic, the sources of this lack of realism, as well as KPD political violence, can now be understood with greater precision.

JAMES M. DIEHL
Indiana University

DANIEL R. BORG. *The Old-Prussian Church and the Weimar Republic: A Study in Political Adjustment, 1917–1927*. Hanover, N.H.: University Press of New England, for Clark University, Worcester, Mass. 1984. Pp. 369. $35.00.

In 1925 the Old Prussian church (Evangelical Church of the Old Prussian Union—that is, of the territories of Prussia prior to the wars of German unification) included 47 percent of the members of the Evangelical Church in Germany. With over eighteen million adherents, it was second among Protestant churches only to the Church of England. Although clichés are difficult to avoid in speaking of it, the church had clearly enjoyed a privileged position in the "power structure" of the German empire. Wilhelm II was its *summa episcopus*, and, like the

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other (Catholic) "church-type" institution (but not the sects), it received salaries for its ministers and a protected position in religious education in the schools.

Then suddenly the Old Prussian Union suffered a drastic loss of power as the *summa episcopus* abdicated and Socialists and teachers attempted to limit church influence in the schools. Until 1927 the union struggled to obtain a favorable school law, but in the end the bill, as amended by the Peoples' party, was unacceptable to the Catholic Center party, which, well enough protected by custom and state law, left the Protestants in the lurch. This should have been no surprise. The Center was notorious for its "opportunism" when it came to protecting the interests of the Catholic church and cooperating with majority Socialists or the Right, depending on the needs of the moment. For their part Evangelicals had supported General Paul von Hindenburg for president in 1925 partly out of detestation for his Catholic Centrist opponent Wilhelm Marx and partly out of "fierce hostility to ultramontanism," which was "perhaps more widespread in the Weimar era than in any other period of modern German history" (p. 262). This is no wonder, for during this period the Center and the Vatican were able to cut their own deals with the state, leaving the former natural rulers embattled (at times) by the Socialists, stripped by the inflation of 1923 of holdings in government securities, and trying desperately to stem the tide of "materialism" engulfing Germany. Later, a segment of Evangelical "war theology" that had taken an ugly turn during the Great War, fed into a *völkisch* theology that was nearly as dangerous. By 1927, at which point Daniel R. Borg chooses to end this work, many church leaders (church attendance was so miniscule that one cannot speak of the laymen) were deeply embittered toward the republican state, which they had never welcomed in any case.

The author seeks to answer the question "Why the Weimar Republic failed to attract the loyalty of Old-Prussian church leaders and active parishioners" (p. xiv). The question might have been better framed, "Why did the leaders of the Old Prussian church hold back from a commitment to the Republic and pine, as some of them did, for an order which, given Germany's social and political conflicts, could never be?" As Borg recognizes, the conservative churchmen of the Old Prussian church (there were a very few Socialists and Democrats) "were part of the problem" (p. 250). Although this is a useful and scholarly book, the question it poses deserves a deeper answer. A Marxist answer would be too simple (conflicts over religion are not simply reflections of class conflict) but might take the analysis further. But beyond that, one needs to know why and how so many Prussian churchmen came to identify their cause, and that of Germany, with God.

LAWRENCE D. WALKER
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ROBERT P. ERICKSEN. *Theologians under Hitler: Gerhard Kittel, Paul Althaus, and Emanuel Hirsch.* New Haven: Yale University Press. 1985. Pp. 245. $20.00.

This is an understanding and carefully documented study of three prominent theological professors who welcomed Hitler's takeover in Germany. It is not a study of theologians in general, although some reference is made to others, particularly to Karl Barth, Paul Tillich, and Dietrich Bonhoeffer. Robert P. Ericksen begins his study with an excellent survey of the main developments in German theological thinking since the Enlightenment. He maintains that "destabilizing elements in the industrial revolution," the rise of democracy, and disillusionment with the belief that rationalism would solve all problems produced a crisis of modernity that was particularly marked in Weimar Germany. Theologians struggled to meet this challenge; the three chosen for study here were all strong nationalists and called for a revitalization and resurrection of the German *Volk*.

Gerhard Kittel was a specialist at Tübingen University in the study of Judaism and its relation to Christianity. As an expert on the Jews, he wrote articles advocating status as guests, not citizens, for Jews in Germany and enthusiastically supported Hitler's arrival to power. He was a charter member of the institute established in 1936 for the study of the "Jewish question." In a statement of defense drawn up while undergoing de-Nazification, he maintained he sought to combat the "vulgar anti-Semitism" advocated by certain party leaders. Ericksen analyzes this statement carefully and traces Kittel's postwar fate. Kittel considered the Nazi party a Christian moral renewal and "never took a clear, public stand either against Nazi policy or for . . . the Confession Church" (p. 49).

Paul Althaus was a prominent Luther scholar and professor at Erlangen University. He was a moderate, a compromiser, who, as so many others, welcomed the so-called national revival promised by Hitler. From the start he turned away from the German Christians, and, although not as ardent in his support as some leaders of the Confessing Church, he definitely was on their side. He at first supported the totalitarian state, but his enthusiasm for National Socialism found no expression after 1937 (pp. 112, 116).

Emanuel Hirsch, the most brilliant intellectual of the three men, was also the most radical. A professor of systematic theology at the University of

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Göttingen, he was greatly influenced by his study of Kierkegaard and existential philosophy. Of the three theologians considered, he was the strongest supporter of Hitler and never did "concede that his political, intellectual, theological position had been wrong" (p. 177).

This is primarily a theological study and is to be warmly welcomed as such. Absent is a consideration of how these men reacted to many of the specific issues—such as the curtailment of civil liberties, the arrest of pastors, the conflict over confessional schools, and the *Kristallnacht* pogrom—that arose during the *Kirchenkampf*. Reference to what position they took toward these problems would have thrown light on how these respected professors could reconcile Christianity with National Socialism. In respect to this problem I treasure the statement by Hans Asmussen, one of the leaders of the Confessing Church: "Without doubt there were people in 1933 who already knew what dangers lay in store with National Socialism. I did not meet such men in 1933. If I early became an opponent of Hitler, it was by God's grace" (*Zur jüngsten Kirchengeschichte* [1961], p. 28).

Yale University Press is to be criticized for running many words together; in several instances virtually whole lines appear as one word.

ERNST C. HELMREICH
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HAROLD JAMES. *The Reichsbank and Public Finance in Germany, 1924–1933: A Study of the Politics of Economics during the Great Depression.* (Schriftenreihe des Instituts für Bankhistorische Forschung, number 5.) Frankfurt a/M.: Fritz Knapp. 1985. Pp. 426.

The title of this book, if understood narrowly, is accurate; the subtitle, however, is an unfulfilled promise, despite Harold James's claim in the brief epilogue that "this study has moved in the area between the politics of economic decisions and the economics of political ones" (p. 348). The politics is an offstage rumbling unrelated to the subjects of such chapters and sections as "Reparations Revision and the Reichsbank" and "The Experts and the Young Plan." James has not dealt directly with any of the increasingly ominous elections of the period, including the destructive one of September 1930, which was fought primarily over the Young Plan and which made the more responsible political parties "guilty of all the destitution and sacrifice" as the plan's defenders. (This quotation is from page 13 of Wolfgang J. Helbich's *Die Reparationen in der Ära Brüning* [1962], listed in the bibliography with other excellent works but, like too many of them, imperceptibly used.)

James's approach to the politics of the period expresses itself chiefly in pejorative epithets and allegations drawn from public controversy. The financier Karl Helfferich, who served prewar, wartime, and postwar Germany greatly if fallibly, is defined away as having "set up the wartime inflation"; Heinrich Brüning is condemned as "the Hunger Chancellor of the Depression"; and "between 1931 and 1933 Germany went about turning herself . . . from an unhappy bad debtor into a laughing one" (pp. 11, 215).

Within the scope of a dissertation James has efficiently organized his data on money supply, reserve ratios, interest rates, and financial agreements made and broken. This material will surely be helpful to other scholars, despite the lack of an index. Yet even in his treatment of Weimar finances James fails to take command of the subject. He begins by introducing the Reichsbank as an obedient collaborator in the government's policy of hyperinflation, goes on to promote it to an institution that "made its own foreign policy" until 1930, and then demotes it to an implementer of a "cynically conceived strategy of foreign political advantage" (pp. 12, 17). At the end, specifying "careful direction of private enterprise," he concludes, without having prepared his readers, that "the Reichsbank had answers to the Slump . . . [that] were . . . more effective and more consistent than has generally been allowed" (p. 347). But, given the economic collapse and the Nazi accession to power, the book is unable to identify listeners to those answers.

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ECKART TEICHERT. *Autarkie und Grossraumwirtschaft in Deutschland 1930–1939: Aussenwirtschaftspolitische Konzeptionen zwischen Wirtschaftskrise und Zweitem Weltkrieg.* (Studien zur Modernen Geschichte, number 30.) Munich: R. Oldenbourg. 1984. Pp. vi, 390.

Most recent historical research has dissolved the conception of Nazi foreign policy as a sequential program set by Hitler. At most, Hitler's personal goals, prejudices, and obsessions set the guidelines, sometimes widening and sometimes narrowing, outside which foreign policy could not stray. Similarly, the idea that the economic foreign policy of either Hitler or the Nazi state was embodied in a definable and distinctive conception of *Grossraumwirtschaft* has also been dissolved.

Eckart Teichert concludes that the idea of *Grossraumwirtschaft* took on so many different forms even within the same governmental circles in the interwar period that it essentially became an integrative po-

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litical formula. After 1930 it attracted increasing support for and loyalty to the Nazi state from other nationalist, conservative, and expansionist groups. Had it been turned into a clear program for action, it could not have served its integrative purpose, so great were the differences in goals of those who continued to use it as a code word indicative of political allegiance. Teichert traces the various uses of this term by using the archives of the commercial section of the Foreign Office, the Ministry for the Economy, and the Reichsstand der Deutschen Industrie. In the economic administration Hjalmar Schacht opposed a purely Continental *Grossraumwirtschaft*, while Hans Posse supported it. Even in the chemical industry, usually said to have been an ardent advocate of expansion into Southeastern Europe, there were powerful voices on the other side. But all used similar language and had in common the goal of making Germany the central major industrial power surrounded by a ring of suppliers of raw materials and markets that were controlled by the same oligopolistic means as in the domestic economy.

The work stands firmly in the Fischer tradition and argues a continuity of expansionist ambition and thought among the German elites that made the Nazi party's task all the easier. It shares with this tradition an almost complete lack of interest in the real workings of the economy, domestic or international, which inevitably makes it superficial and vitiates much of its argument. At times it reads as though German businessmen were more interested in diplomacy than business. Readers should be warned in addition that it is a work of almost infinite pedantry and tedium. Its 272 small pages of text refer to 1,587 footnotes, many of them to sentences so insignificant or otiose that the reader will need considerable motivation to get to the next.

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GERHARD SCHREIBER *et al. Der Mittelmeerraum und Südosteuropa: Von der "non belligeranza" Italiens bis zum Kriegseintritt der Vereinigten Staaten.* (Das Deutsche Reich und der Zweite Weltkrieg, number 3.) Stuttgart: Deutsche. 1984. Pp. xii, 733. DM 78.

This book, the fourth of ten projected volumes in the West German series Das Deutsche Reich und der Zweite Weltkrieg, enhances both the quality of the series and the reputation of the *Militärgeschichtliches Forschungsamt*. Like its predecessors, *Der Mittelmeerraum und Südosteuropa* takes a strong and controversial position. This time, however, the myths being challenged are British and American as well as German. For Gerhard Schreiber and his colleagues, World War II was essentially a conflict of economic resources. This in turn makes nonsense of the argument, so often repeated in English-language accounts, that Nazi Germany in 1940–41 missed a major opportunity to turn the war in its favor by concentrating on the Mediterranean. Questions such as what Rommel might have done with two or three additional panzer divisions fail to recognize that the Mediterranean was from the beginning a strategic dead-end for the Axis.

The error has been enhanced by legends of a clean and chivalrous war in the deserts of North Africa—legends fostered, on one hand, by the German interest in turning attention away from the atrocities on the Russian front and, on the other, by the British desire to highlight the empire's last stand as an independent power. Britain's refusal to consider a negotiated peace in 1940 may indeed have been as close to a turning point in World War II as the fundamental imbalance between the combatants allowed. But Britain's successful stand ultimately depended less on its Mediterranean position than on the moral and physical support of the United States. As long as this was sustained, Britain could not be driven out of the war no matter how great the pressure on the alleged imperial lifelines.

Seen in this context, Hitler, far from fecklessly ignoring the Mediterranean, fitted it with reasonable accuracy into his ideologically determined grand strategy. For him the decisive enemy was always Russia; the decisive theater was always the East; the decisive goal was always *Lebensraum*. It was the military, specifically Admiral Erich Raeder, that looked with longing at the prospect of cheap victories in Southern Europe. For his part Hitler had no patience with a situation demanding, above all, careful management if it was to show even marginal results. Spain's refusal to enter the war; Hitler's refusal to consider honest cooperation with Vichy France; and, most important, the tensions between Berlin and Rome—these conditions in themselves limited Axis opportunities in the Mediterranean to regional conquests and tactical victories, draining already scarce resources and straining an already shaky alliance.

Also central to the work's argument is its refutation of Mussolini's opportunism. The fascist dictator appears in these pages as pursuing a consequent policy of expansion and aggrandizement virtually from his accession to power. This view is supported by a detailed analysis of Balkan and Mediterranean politics in the interwar years. Schreiber clearly shows that Italy's declaration of war on France, the attack on Greece, and the invasion of Egypt were short-range steps toward long-range imperial goals. Mussolini had no more desire to see German hegemony in Europe than to take second place to Britain or France. The limited Axis planning for a coalition

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war reflected in large part Italy's determination to pursue an independent policy. This policy, in turn, fatally overstrained a military capacity itself limited by an underdeveloped economic, industrial, and financial structure. And Italy's appeals for help in the spring of 1941 were appeals Nazi Germany could not afford to ignore.

The volume as a whole exemplifies the new military history's integration of economic and political factors, short- and long-term diplomatic considerations, and more conventional operational material. Yet traditionalists who might believe that actual fighting is neglected in terms of pages must respect the quality of the offerings. Schreiber's discussion of the Italian operations in Greece is the best brief summary in any language. Bernd Stegemann's section on the North African campaign is a model of theater history. And the *Militärgeschichtliches Forschungsamt* deserves special praise for its efforts to move away from the heroic vitalism that characterizes so much of the writing about this period of the Mediterranean campaign.

One problem remains. No one told the British in 1940–41 how objectively well off they were. War remains a factor of will as well as material. The moral and political effects of a collapse of Britain's Mediterranean front in Britain itself, in the empire, and, not least, in the United States were not likely to have been entirely determined by the kinds of calculations developed at the desks of academics with the advantages of forty years' research in open archives. Given that fact, the temptation will remain among soldiers, scholars, and buffs alike to call for another round of drinks and ask themselves, "What if?"

DENNIS E. SHOWALTER
Colorado College

WOLFGANG JACOBMEYER. *Vom Zwangsarbeiter zum heimatlosen Ausländer: Die Displaced Persons in Westdeutschland, 1945–1951.* (Kritische Studien zur Geschichtswissenschaft, number 65.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 323.

Of all the problems the western armies had in their drive to destroy the Third Reich, the least expected was the problem of the displaced persons (DPs). As the western armies approached the frontiers of the Reich in 1944, they picked up a quarter million DPs, but this was nothing compared to what they found in the Reich itself. By the end of the war they had inherited the staggering total of 11,332,700 such refugees. The care and disposition of these millions of DPs presented not only technical and administrative problems of great magnitude but complex political problems as well. The western armies were ill-prepared to handle these gigantic problems, and so were the civilian agencies such as the Red Cross and United Nations Relief and Rehabilitation Administration. In a carefully crafted study Wolfgang Jacobmeyer traces the evolution of the DP in the three western zones of Germany from the war until 1952. What he found was not pleasant. The history of the DPs is another of the unknown tragedies of the war.

The first response of the occupational authorities was to collect, care, and control the DPs. This was no small task since the living circumstances of many of them were appalling and since some were bent on revenge. Political problems occurred immediately. Many of the DPs whose former homes were in areas controlled by the Soviets refused to return and wanted political asylum. Many of the Soviet citizens wanted to remain too, but their fate had already been decided by prior Allied agreements; they were forcefully repatriated in one of the saddest episodes of the postwar years. As for those DPs who wanted to go home, the Allies made swift work of them. By the end of September 1945 about 5 million got home on their own while another 5 million were shipped home, leaving 1.2 million. Of these, roughly three-quarters were either Polish, Jewish, or Baltic DPs who were not to be repatriated but had to be resettled. This group constituted the "hard-core" DPs that remained in the camps for many years.

Jacobmeyer is at his best describing the difficulties in reducing the hard-core DPs in the western zones and the changes in the attitudes toward them. The legal, political, and economic problems were complex and vexing to both the Allies and the Germans. More interesting are the subtle changes in the stereotype of the DP, from a victim of the Nazis to a lazy, unwanted troublemaker living in "welfare camps." By 1947 the Allied authorities were trying to force the DPs to work while also employing stiff police measures against them in the camps. Differences with UN agencies compounded the problem, as did the growing camp mentality of the DPs. Eventually the total of DPs was reduced to 200,000 by 1950, when in effect the Allies dumped this problem on the newly created West German state.

The author has primarily used documents from the UN and from the Allied governments with effectiveness. His account of the international bureaucratic bungling of the DPs by the Allies, the UN, and the Germans during the early years of the cold war is not very edifying, but it does explain another of the "minor tragedies" of World War II and its aftermath.

EDWARD L. HOMZE
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RUDOLF BRAUN. *Das ausgehende Ancien Régime in der Schweiz: Aufriss einer Sozial- und Wirtschaftsgeschichte*

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des 18. Jahrhunderts. Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 328. DM 68.

Rudolf Braun's structural sketch of the Swiss *ancien régime* aims primarily at historians of Switzerland and intends to bring about a change in the teaching of Swiss history by introducing an element of dynamics into a static historical self-image. This book is, however, also for students of early modern Europe in general, for, besides presenting a kaleidoscope of people, institutions, and ideas going through systematic and interrelated changes, it also provides a wealth of information for further comparative studies. The author's sources are the writings of several Swiss moral economists and "economic patriots" of the eighteenth century and the monographic literature since 1900, especially recent, often unpublished, work by young Swiss historians.

The book is organized into six thematic chapters dealing with population development and the "family constitution," agrarian systems and agricultural production, rural putting-out industries, urban social culture, administrative politics, and, finally, social and political conflicts as "forerunners of the great turning point." Braun weaves together many comparisons and conclusions throughout the book; thus, it is difficult to isolate a single line of argument. It appears that the narrowing and closing of both urban and rural elite circles around 1700 placed a firm cage around the Swiss economy and society just as they were entering a century of growth and change spurred by the business cycle and by burgeoning urban and rural industries. The strains of this simultaneous growth and stagnation were felt at every level of social, intellectual, and political life and even the period of both domestic and imported revolution at the end of the century could not relieve the accumulated tensions.

That the explanations Braun offers for the unresolved contraditions at the end of the *ancien régime* do not finally satisfy results from his failure to resolve the contradictions between his information and his conceptual language. For example, although the second chapter contains an excellent analysis of the interplay between changing regional agricultural specialization and rural industrialization, it does not effectively support the book's main argument. This is because Braun persists in using the term "protoindustrialization," with all its implications of a forward-looking, growth-oriented economic and social evolution. The facts he presents clearly demonstrate that rural industrialization functioned to secure more profitable agricultural enterprises and to stabilize the communal life and conservative social politics in areas of impartible inheritance (compare Eckart Schremmer in *Geschichte und Gesellschaft*, 6 [1980]).

Such conceptual inadequacies and uncertainties plague the book throughout. It is difficult to see a humanist like Braun use terms like "human resources" (p. 51) and "labor capital" (p. 113). He accepts, moreover, Otto Brunner's language about the early modern family even when it proves inadequate for conceptualizing the deterioration of families into aggregations of people in which "everyone has to look out for himself" (pp. 209, 134–35). With his denial that those who lived and labored under the various systems of rural industrialization had their own system of values (*eine Lehre*) Braun seems to turn his back on his earlier work, in which he made such a valuable beginning toward a history of the mentality of these social groups. This study records the growing tensions within an intellectually and practically failing system of moral economy, and the full experience of the victims of that failure cannot be left out without damaging the explanatory value of the book. In the end we are left with conspiratorially organized urban rebels worrying about "status inconsistencies" and "blocked chances for mobility" (p. 271) and disputing with patrician governors about the aristocratic and democratic tendencies of accepted corporate systems.

Nonetheless, this book tells many interesting stories and offers a powerful synthesis of a vast array of scholarly work. It is an important and revealing stocktaking of the historiography of a people whose historical experience has long been concealed behind traditional claims of communal stability, prosperity, and order.

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GIOVANNI LEVI. *Centro e periferia di uno stato assoluto: Tre saggi su Piemonte e Liguria in età moderna*. (Storia, number 38.) Turin: Rosenberg e Sellier. 1985. Pp. 226. L. 21,000.

This volume, a collection of articles and chapters published by Giovanni Levi from 1971 to 1978, makes his work more readily available in expanded or revised form. The focus of his attention is Piedmont and that section of Liguria controlled by the House of Savoy. The time span is the seventeenth and eighteenth centuries.

His first chapter describes how the growth of absolutism led to the concentration of power in Turin. Its title, "Come Torino soffocò il Piemonte," is indicative of Levi's thesis. A large number of towns (*città*) that had grown up around the capital suffered declining population from 1614 to 1734. Turin revived them by becoming the chief market for local specialized produce. Their revival required that they become intermediaries between the countryside and Turin; their populations grew, but the

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towns in turn became mere satellites because Turin, as the dominant power, was able to organize them to satisfy its own needs. Levi is not here concerned with urban history as such but with the changing pattern of migration, and his work is mainly demographic. He finds that rural folk were more likely to move permanently to Turin whereas those from other towns and their countrysides moved seasonally, working as laborers for part of the year, then returning home for a brief sojourn until their next seasonal migration.

The second chapter is a study of peasant families in fifteen communities in the province of Oneglia. Levi uses demographic methods and sources in order to discover how families are formed, their strategies for survival, and their structures. Levi contributes to the concept that the life cycle of the family is dynamic and changing; therefore, the family may be nuclear at one time and more complex at another. A domestic group must restructure its landholdings, work, wealth, and size to survive two difficult periods: first, when children consume and do not work and, later, when old parents consume and cannot work. Levi has discovered numerous extended families. It was, he argues, necessary to keep or add members, even servants, in order to make certain that producers were more numerous than consumers in the household. Both size and productive capacity even influenced culture because large families could allow one child to continue in school beyond the age of twelve.

The third chapter investigates relations between landed property and family structure as well as relations between clientage and marriage strategy in the large village of Felizzano, located in the cereal-growing plain of Piedmont. The marriage age of a girl was not decided simply by fecundity; the amount of land required for her dowry and the time needed to obtain this land were also determinants. The social level of the affianced couple had to be maintained. Endogamy was widely practiced, so land was rarely alienated to outsiders and its exchange was not intended to enrich one family over another but to preserve status and ensure survival. For this reason rich girls did not marry poor men, whatever their hearts might long for. In this chapter Levi also modifies his use of essentially quantitative methods to describe in detail the rise and fall of a notable, the clientage network of a family, and the failure of a wealthy outside merchant to create a position of power in Felizzano.

I highly recommend this book to serious scholars interested in migration and family history and their evolution over time. Levi provides what he calls a "processo generativo," a method that interprets the past as processes rather than as static conditions. He has explored vast archival sources, mostly quantitative, to give us glimpses of the quality of life several centuries ago.

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CARLO M. TRAVAGLINI. *Analisi di un'agitazione contadina nella campagna romana all'epoca della Restaurazione.* (Pubblicazioni dell'Istituto di Storia Economica.) Rome: Università degli Studi or the Istituto di Storia Economica, Rome. 1984. Pp. 125.

In Italian history the Restoration (1815–48) is studied mostly as a phase in the movement for national independence and unification. As a result, we know much more about the politics of nationalism and the role of the Italian question in international diplomacy than about the economic and social history of Restoration Italy. In spite of important work done by several scholars, including Marino Berengo, Franco Della Peruta, Carlo Pazzagli, and Guido Quazza, who have examined the state of the economy and society in different parts of the peninsula, it is still easy to forget that, in Italy as elsewhere in Europe, the age of political restoration was also an age of economic and social change. Carlo M. Travaglini now reminds us of that in this brief but well-documented and suggestive study of a peasant protest that broke out in the Roman *campagna* in May 1832.

Travaglini reconstructs this obscure episode by pursuing a trail that takes him through various Italian and Vatican archives. The protest began with a small group of peasants who worked on large estates and met in secret to draw up a written list of grievances that demanded redress for low wages, inadequate food, and mistreatment by their immediate supervisors. A core of organizers (fifty-three were subsequently arrested) took the initiative, but the movement spread within a few weeks until it involved much larger numbers. The organizers not only drew up a list of demands but also worked out a strategy of protest that ruled out the use of violence against people and property and made clear that they were protesting actions taken by their employers, who were "country merchants" leasing and managing estates owned by religious bodies and members of the Roman nobility. Although they referred to their movement as a "revolution" (*revolsione*), the peasants made clear that they were not challenging the state and, indeed, hoped that the authorities would take their side against the employers. Perhaps to their surprise, that is precisely what happened. Government officials and Pope Gregory XVI condemned the employers for their callousness, called for immediate relief, and sent the arrested peasants back to their estates. The

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reasons for this favorable response not only included fear that repression might lead to more agitation but also reflected a sense of justice based on the Christian principle of just prices and wages. Not much was accomplished beyond providing some immediate relief. The opposition of country merchants and the Rome Chamber of Commerce stymied government efforts to deal with the root causes of rural unrest.

One could perhaps conclude that, in an emerging capitalist economy like that of Rome in the 1830s, the pope proposed while the merchants disposed. That conclusion can certainly be inferred from Travaglini's work, but it is not the one he stresses. He relates this incident of peasant militance to the literature on modernization and popular protest, comparing the characteristics of the episode in question with those of popular protest movements studied by Eric J. Hobsbawm, George Rudé, E. P. Thompson, Louise A. Tilly, and others. He warns that on close inspection few movements fall into preordained categories but suggests nevertheless that this particular movement had more in common with industrial than with preindustrial forms of popular protest. Indeed, why not! The more we study peasants, the more likely are we to discover unsuspected aspects of modernity in their culture.

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DENIS MACK SMITH. *Cavour*. London: Weidenfeld and Nicolson. 1985. Pp. xii, 292. £12.95.

ROSARIO ROMEO. *Cavour e il suo tempo*. Volume 1, *1810–1842*; volume 2 (in two parts), *1842–1854*; volume 3, *1854–1861*. Rome: Laterza. 1969–1984. Pp. xvi, 822; xv, 911; xi, 989. L. 7,500; L. 18,000; L. 60,000.

Denis Mack Smith has written during the past three decades numerous provocative books on Italy's Risorgimento: *Cavour and Garibaldi, 1860* (1954; republished in 1985 with a new preface), *Garibaldi: A Great Life in Brief* (1956), *The Making of Italy, 1796–1870* (1968), and *Victor Emanuel, Cavour, and the Risorgimento* (1971). He has also dealt with other aspects of Italian history: *Italy: A Modern History* (1959; 1969), *Medieval and Modern Sicily* (1968), *Mussolini's Roman Empire* (1976), and *Mussolini* (1982).

Readers familiar with Mack Smith's *Cavour and Garibaldi, 1860* will recall that he was often quite critical of the Piedmontese prime minister's pragmatic and sometimes ambiguous role in the Risorgimento and especially of his devious moves to checkmate Garibaldi, who was clearly Mack Smith's favorite. In his most recent book, however, Mack Smith has softened some of his earlier judgments of Cavour and now credits Cavour with accomplishing in a short life span of fifty-one years more than any other European statesman did in that era—all the more remarkable because Sardinia-Piedmont was a small state with limited resources. Indeed, no politician of the century—and certainly not Bismarck—was able to achieve so much with so little. Mack Smith goes on to argue that, thanks to Cavour's political virtuousity, Italy's liberal parliamentary system was established and a difficult and even paradoxical goal for Italy was attained—a "conservative revolution" rather than the more idealistic and democratic outcome that Mazzini and Garibaldi envisaged. During his last months Cavour challenged excommunication and declared war on the pope, thus opening the way to what Mack Smith regards as the final and most noteworthy victory of the Risorgimento: the destruction of the papacy's temporal power, a power that had survived intact for so many centuries.

These triumphs, the author explains, were achieved in the face of adversities of every sort. Cavour had to overcome the difficulties raised not only by the domestic opposition within Piedmont but also by all of the other Italian states. One of the results, Mack Smith observes, was that the Risorgimento, far from being simply a struggle against foreign oppression, was also a series of civil wars that left festering wounds. On balance, however, he sees the unification as a great success. Although Cavour sometimes stood on the brink of total failure, made errors of judgment, and employed unscrupulous methods, the Piedmontese leader had the ability to rectify his mistakes and to exploit adverse circumstances to his own advantage, thereby displaying the talents of a supreme statesman.

Mack Smith's 294-page biography is composed of twenty-four fast-paced chapters and a valuable bibliographical note. Unfortunately, there are no footnotes. The author chooses to concentrate on Cavour's public life. Thus, in the initial chapter he quickly summarizes the first twenty-four years of Cavour's life, while in the second he explains how Cavour's visits to liberal Britain and France between 1834 and 1841 helped shape his economic and political philosophy. The third chapter discusses Cavour's apprenticeship in public life (1841–47). Most of the others deal with shorter time spans, in some cases only a few months. One interesting chapter sheds light on the life style of the mature statesman. Although he never married, Cavour had at least two mistresses, whom he prudently kept out of public view. Often moody, he sometimes erupted into violent rages, and during one heated argument with King Victor Emmanuel he even broke a palace clock.

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In his bibliographical note, Mack Smith—whose previous acerbic evaluations of Cavour have more than once made him the target of sharp criticism from Rosario Romeo and other Italian liberal historians—generously pays his respects to Romeo's huge study, *Cavour e il suo tempo*, the last volume of which appeared when the British scholar's book was in press. He points out that Romeo's biography is really a wide-ranging history of the era, which focuses especially on the economic background of Piedmont and on the diplomatic framework in which Cavour acted. Although noting that some of Romeo's conclusions differ from his own, Mack Smith acknowledges that the Italian scholar's biography will probably stand for some time as the most exhaustive treatment available.

Romeo belongs to the liberal, Crocean "ethic-political" school of historiography but has been favorably impressed by both the *Annales* school and American quantification methodology. Romeo quickly achieved acclaim for his youthful study, *Il Risorgimento in Sicilia* (1950). Thereafter, he turned his attention to writing, among other things, a *Breve storia dell'industria italiana* (1961; 1967) and a collection of critical essays, *Risorgimento e capitalismo* (1959). These publications vigorously rebutted Antonio Gramsci's contention that the Risorgimento was, in essence, a "failed agricultural revolution." In view of Romeo's predilection for economic history, it is not surprising that his biography of Cavour focuses heavily on "the times."

The first installment of Romeo's massive work appeared sixteen years ago and dealt with the years 1810–42, the period prior to Cavour's entry into public life. Each of its 823 pages is studded with footnotes, many of them citing material in the Cavour family archives that hitherto had never been used. That initial volume is remarkable for its perceptive investigation of the economic, political, and intellectual background of Cavour's family during the period of the French Revolution and of the Napoleonic occupation of Turin. Thereafter, the author discusses in detail young Cavour's spiritual development, his career in the army engineer corps where he was exposed to progressive ideas coming from Genoa, his increasing admiration for British and French institutions and economic thought (especially free trade), his interest in railroad construction and the stock market, and his determination to introduce scientific management of his estates.

Romeo's second volume, published in two tomes, came out in 1977. It starts with the establishment of the Associazione Agraria Subalpina in Turin in 1842, in which were to be found the most influential political currents in the future life of liberal Piedmont. Next, the author discusses Cavour's role as editor of *Il Risorgimento* during the liberal-nationalist revolutions of 1848–49, and his subsequent rise to the office of prime minister in 1852. This volume concludes with the outbreak of the Crimean War. The narrative is often interrupted by long digressions in economic history that might better have been published elsewhere.

Romeo's final volume rests on much archival research in Italy and abroad. It brings us to the climactic phase (1854–61) of the Risorgimento, when Premier Cavour, keenly aware of the need to enlist the support of France in expelling Austria from northern Italy, succeeded in working out a bargain with Napoleon III to this effect. Cavour did not get all that he hoped for out of the ensuing war of 1859, because Sardinia-Piedmont was permitted to acquire only Lombardy. Through subsequent skillful maneuvers, however, Cavour overcame Napoleon's reluctance and also annexed Modena, Parma, Tuscany, and Romagna. Romeo presents a masterful analysis of all of these complex moves as well as of the strained relationship between Cavour and Garibaldi in 1860. The author is particularly persuasive in his explanation of how the speed of Italian unification in the years 1859–61 caused Cavour to opt for a centralized rather than a regional administrative system in 1861—his last major decision before his untimely death in June.

In contrast to Mack Smith's succinct and elegantly written biography, Romeo's careful analysis of the many controversial aspects of Cavour's life is addressed primarily to specialists. Those who have the time to plough through his lengthy work will be rewarded with a wealth of new information that has been carefully interpreted.

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ANTONIO CARDINI. *Antonio de Viti de Marco: La democrazia incompiuta, 1858–1943*. (Collezione di Studi Meridionali.) Rome: Laterza. 1985. Pp. xii, 386. L. 43,000.

In 1930 Umberto Zanotti-Bianco and Ernesto Rossi published an anthology of the political writings of Antonio de Viti de Marco entitled *Un trenennio di lotte politiche (1894–1922)*. It remains the principal printed source for the political thought of this economist, academician, polemicist, political figure, and southern apologist. Antonio Cardini's work seeks to supplement this earlier one, outlining de Viti de Marco's biography and reconstructing in a systematic manner the development of his thought. Although the dates of Cardini's study are 1858–1943, from de Viti de Marco's birth to his death, this is not a traditional biography but a study of the public figure.

Within the pages of the biographical note and the first chapter, Cardini traces the early years of the

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protagonist, focusing on those aspects of his private life that impinged on the formation of his ideas. Among other things he notes that his subject's Neapolitan mother, Lucia Troysi, had British ancestors. Cardini cites both the importance of this southern background and the Anglo-Saxon connection in de Viti de Marco's formation. The first is well known, the latter less so, and Cardini makes a contribution by stressing it. The Anglo-Saxon influence was personal and intellectual since the economist read the works of Adam Smith, David Ricardo, and John Stuart Mill and discussed the theory of marginal utility of William Stanley Jevons with his friend and collaborator, Maffeo Pantaleoni, who had a British mother. Later, de Viti de Marco married Harriett Lathrop Dunham, an American of English origin from New York, who contributed to making him an Anglophile. English liberal and free-trade currents provided de Viti de Marco with the means to criticize governmental policies as he sought to resolve the problem of the *Mezzogiorno*.

Cardini's book concentrates on de Viti de Marco's crusade against the philosophy and practices of Liberal Italy from 1896 to 1918; the final chapter reveals why he found fascism equally unacceptable. In chapter 2 the author specifically examines the economist's evolution of a liberal-democratic ideology based on the Anglo-Saxon model; chapter 3 focuses on de Viti de Marco's campaign for an antiprotectionist movement that would uplift the south; chapter 4 delves into his critique of Giovanni Giolitti's program between 1905 and 1911; chapter 5 examines the relationship between radicalism and imperialism in Italy from 1911 to 1914; chapter 6 reveals the differences between the free-trade liberals and the protectionist nationalists during the course of the world war. Throughout, Cardini presents de Viti de Marco's position concisely and draws on his correspondence as well as his writings in the free-trade *Giornale degli Economisti* and Gaetano Salvemini's *L'Unità*.

Although Cardini provides an excellent account of de Viti de Marco's critique of the economic and political policies of Giolittian Italy and a clear picture of his proposed alternative, an incisive analysis is lacking. How relevant were English free-trade policies and party practices to Italy at the turn of the century? Was there a strict connection between protectionism and imperialism as de Viti de Marco claimed? Did the protectionism of Liberal Italy contribute to the southern problem? Could the south have been uplifted by the free-trade program that the economist demanded? Finally, and perhaps most important, was de Viti de Marco's trenchant criticism of Liberal Italy constructive or destructive? What part, if any, did it play in the collapse of the parliamentary state and the triumph of fascism? These questions are neither posed nor answered.

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MARK F. BLUM. *The Austro-Marxists, 1890–1918: A Psychobiographical Study*. Lexington: University Press of Kentucky. 1985. Pp. 254. $28.00.

The generation of brilliant Austrian Social Democratic theorists and political leaders who built the Marxist movement before World War I were later to play vital roles in the political life of the First Austrian Republic and the Austro-fascist period. They were gifted ideological critics of the Habsburg empire and creative shapers of international socialism. This astute and talented group of European socialist thinkers included Karl Renner, leader of the right wing; Otto Bauer, leader of the left wing; Max Adler, a party ideologue; Friedrich Adler, radical son of the party's founder, Victor Adler. Renner and Bauer were distinguished in that they were both parliamentary leaders and substantial intellectuals who wrestled with the complexities of the nationalities question that was to tear the Dual Monarchy apart and ignite the First World War.

Mark F. Blum gives us a psychological portrait of this significant group of Austro-Marxist leaders. He re-creates their social matrix through an analysis of the party oligarchy and the intellectual context with excursions into philosophy, ontology, and phenomenology. Blum's theses about Renner and Bauer are plausible: Renner sought a stable home in the party, and Bauer found in the party a loving family. In the case of Friedrich Adler, it is doubtful that the party served him as a father. Indeed, in handling the climactic event of Adler's life—the assassination of Austria's prime minister in 1916—Blum never adequately tells us why Adler committed the murder and what it meant to him emotionally.

The book has the merit of dealing with these important Austro-Marxist leaders on whom very little has been written in English. It does not discuss them, their relationships and interactions, as a group. Throughout, Blum's treatment has a softness and vagueness that left this reader frustrated. The book bears too many of the rough edges of a doctoral dissertation. Matters that require emphasis because of their structural or historical import are lost in the text.

Originally a lengthy, rich, enterprising, and sometimes insightful 1970 University of Pennsylvania doctoral dissertation, this manuscript badly needed good editorial work that it did not receive. Freud is cited from the old *Collected Papers*, not from the *Standard Edition*, which has been "standard" for the

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past decade. The style is often obfuscatory rather than structured for narrative clarity. Friedrich Adler as the assassin of Count Sturgkh, the Austrian prime minister, is thrown in as an aside in parentheses (p. 175). Important political transitions, such as the Provisional Assembly that met to draft a new constitution on October 21, 1918, are slipped into the middle of a paragraph (p. 178). Blum footnotes a German-English dictionary for the meaning of "Hammer" (p. 238 n. 5). To claim that Friedrich Adler spent "the duration of the war at his father's side" after his release from prison and their meeting on November 2, 1918 (p. 205), is misleading since the "duration" for Austria was a little over a week and Victor was to die on November 11, 1918.

I have strong reservations about Blum's use of psychodiagnostics. He expands the use of the category "hysteric" to the point of making it meaningless. Otto Bauer is inappropriately labeled an hysteric, when, according to the overall picture and Blum's own reading of the evidence ("hyperintellectuality," "compromise and passivity to events," "verbal and behavioral equivocation" [pp. 74, 184]) the style of the obsessive-compulsive is clearly indicated. Blum also calls Friedrich Adler and all of the Adler family with but one exception "hysterical" (p. 234 n. 6).

The book is least satisfactory in its treatment of the Adler family. Blum failed to consult the important Emma Adler 1930 manuscript, "Biographic Victor Adlers," which describes her relationship to her husband, Victor, and her son, Friedrich. For any biography, this memoir is a rich and essential source of the family, that tells, for example, of a shot, fired "accidentally" by Victor Adler, that almost killed his wife and of a consistent pattern of denial of important matters such as financial distress and aggression.

Blum could have done much more with the materials he did have. By not drawing conclusions and anchoring data in interpretation that gives them meaning, he leaves the reader with an unsatisfied sense of being afloat in the Sargasso Sea. He makes nothing out of the fact that Emma Adler offered a joint suicide pact to her son in the first days of the war in 1914 or of her support and identification with her son against his father. Blum extensively quotes Victor Adler's protest that his struggle with his son was not personal; Blum leaves it at that as though there were nothing more to say, add, or interpret.

Blum engaged with extensive materials and saw the Austro-Marxists' vision and many of their problems. In this case the dissertation was better than the book.

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WIKTOR SUKIENNICKI. *East Central Europe during World War I: From Foreign Domination to National Independence*. In two volumes. Edited by MACIEJ SIEKIERSKI. Foreword by CZESLAW MILOSZ. (East European Monographs, number 119.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. xv, 654; 655–1228. $60.00 the set.

The reader hoping to learn about the activities and fate of Czechs, Slovaks, Magyars, Serbs, Croats, Slovenes, and Romanians during World War I by reading this mistitled book will be sorely disappointed. But those interested in the Baltic peoples (Lithuanians, Latvians, Estonians), Eastern Slavs (Belorussians, Ukrainians), and Poles residing between Germany and Russia—in short, the territory of the old Polish-Lithuanian Commonwealth—will find in these two volumes a wealth of information that outlines the fortunes of those nationalities from the beginning of the twentieth century to the conclusion of the formal armistice in November 1918. Wiktor Sukiennicki, a native of Lithuania, scholar in prewar Poland, and a long-time member of the staff at the Hoover Institution on War, Revolution and Peace at Stanford University, has devoted his postwar professional activities to compiling a history of those tumultuous times and their impact on his homeland. The result is a detailed mass of data that future students desiring to learn more about this frequently neglected aspect of World War I will find invaluable as a springboard for further research.

This massive work has much to commend it to the serious scholar. Its size alone—over twelve hundred pages, of which nearly three hundred are detailed footnotes—attests to the depth and breadth of research that the author invested in this book. He consulted virtually every published primary source and secondary work, along with most pertinent available archival deposits, and has quoted much of this material at length. Although the Germans, Russians, and Poles—the major actors in this drama—understandably draw the most attention, the other peoples of the region receive extensive treatment; in fact, Sukiennicki accords separate sections to the Ukrainians, Belorussians, and Balts and discusses the influence of key developments on them. Moreover, he treats the various nationalities in equally objective and impartial fashion, as noted by Nobel Prize winner Czeslaw Milosz in his brief preface.

Unfortunately, several major flaws seriously mar this otherwise superb piece of scholarship. Most of them are structural in nature, items that editing should have removed. The book is poorly organized, with the final two chapters of part 5 spilling over into the second volume and little logic in the placement of chapters, which often do not follow

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one another coherently. Many of the direct quotations are far too lengthy, and several strike the reader as superfluous—the sort of feature found in the first draft of an undergraduate thesis whose author is determined to display how much material has been gathered. Even the footnotes are verbose, often including direct quotations and narrative that should have been eliminated or placed in the text. The index lists individuals only, excluding key subject and topical entries, and is difficult to use. There are also some glaring stylistic shortcomings, most notably, awkward syntax and a nearly total lack of transition within and between chapters. Finally, and most serious, the entire book is almost pure narrative, with little attempt to analyze or interpret the events and developments described in such excruciating detail. A concluding chapter, for example, would have effectively summarized the four years treated in the preceding thirty-two chapters and explained their significance for the two decades to follow. Instead, the text ends abruptly with the conclusion of the armistice, which was merely a prelude for most of the peoples involved in the author's story.

These deficiencies all combine to produce a work that is ponderous, frustrating, and at times confusing and irritating to read. Although the flaws detract from its attractiveness, this book nonetheless belongs on the reading list of anyone interested in obtaining a detailed look at developments in this part of the world during the First World War.

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GYÖRGY PÉTERI. *Effects of World War I: War Communism in Hungary*. (War and Society in East Central Europe, number 16; East European Monographs, number 167; Brooklyn College Studies on Society in Change, number 35.) Brooklyn: Social Science Monographs, Brooklyn College Press; distributed by Columbia University Press, New York. 1984. Pp. x, 229. $25.00.

Communism, said Lenin, equals the Bolshevik Revolution plus the electrification of the country. Of course, as surely even Lenin knew, matters were considerably more complicated than that in Soviet Russia and have been in every country where Communist regimes have successfully maintained themselves in power. In the case of the short-lived Hungarian Soviet Republic of 1919, electrification was the least of its leaders' worries. In addition to armed intervention by numerous enemies, an attempt to transform the noncapitalist economy overnight into a "socialist" one occupied much of their energy. This book by György Péteri provides a brief but painstakingly researched analysis of this ill-fated effort.

The title of the book is a bit misleading. As the author himself notes, he is not interested in compiling a balance sheet of the merits and demerits of the Hungarian Soviet Republic as a whole, nor does he explore the effects of World War I as such. Rather, he offers a sophisticated and almost microscopic examination of the system of industrial production and management introduced in Hungary in 1919, including an analysis of the mechanism for expropriation of capital, trends in employment and productivity, the phenomenon of economic regionalism, and the nature of the industrial firm. Along the way he provides a balanced description and assessment of the many conflicts between the economic theories of the Hungarian Communists, on the one hand, and actual management and production practice as they developed on the shop floor, on the other.

Indeed, such conflicts provide one of the book's major fascinations, for it comes as something of a surprise to discover that many of the tensions apparent in Lenin's Russia had their analogs in Soviet Hungary—grass-roots spontaneity versus centralized state direction, worker radicalism versus regime conservatism, autonomy of worker organizations versus subordination to the demands of the party-state, workers' control versus productivity. A signal virtue of Péteri's volume is that it places these tensions in a nonjudgmental context. He is concerned with analyzing causes, describing conflicts, and observing actual, or interpolating probable, effects, not with superimposing some ex post facto ideological straitjacket on his material.

Yet Péteri may have had a hidden agenda, after all, in undertaking this study. Contemporary Hungary is in many respects in the throes of the same sort of intense and potentially momentous debate about the character and overall direction of its economy as was its 1919 counterpart. More than once, the author seems to have his eye on the current debate while ostensibly focusing on earlier economic conflicts. It is tempting to speculate on what purpose he may have had in mind and on whether he is in fact serving as some contemporary party faction's stalking horse. Unfortunately, one can but speculate, since the study is bereft of any biographical information about the author, even a notation of institutional affiliation on the title page.

The book has more serious problems than this, however. The technical vocabulary is often too rarefied for the nonexpert; a glossary of specialized terms would have been most welcome. Another maddening impediment to comprehension could have been easily remedied by better editing—paragraphs, into which long direct quotations are frequently embedded, often run on for several pages. A particularly regrettable lacuna is the absence of a bibliography and index, although this shortcoming

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is partly compensated by exhaustive notes at the end of each chapter and, at the end of the book, excellent brief biographies of the *dramatis personae*. Finally, the generally serviceable translation from the Hungarian original is marred by occasional Hungarianisms in word order and syntax.

Despite these technical flaws, however, this book remains an admirable piece of research. Péteri now joins the ranks of Hungarian revisionists who for some time have been bringing balanced, rigorous, and informed scholarship to bear on the antecedents, personalities, and outcomes of the revolutionary period of 1918–19. His careful, dispassionate work of analysis of the industrial economic structure of Hungarian War Communism, although a bit too arcane for the general reader, should prove enormously interesting as well as useful to specialists in European economic history or socialist economic theory.

WILLIAM M. BATKAY
Montclair State College

MICHAŁ JERZY ZACHARIAS. *Polska wobec zmian w układzie sił politycznych w Europie w latach 1932–1936* [Poland in the Face of Changes in the Structure of Political Powers in Europe in the Years 1932–36]. Wrocław: Ossolineum. 1981. Pp. 300. 180 Zł.

The policies of Józef Beck, the foreign minister of Poland from 1933 to 1939, are still the subject of historical studies and interpretations. Beck believed that the security of Poland had to be based on her alliance with France and England and that it was in the Polish interest to follow an even-handed policy toward Moscow and Berlin so that Poland could stay independent of both. In this study Michał Jerzy Zacharias provides us with an interesting thesis. He argues that 1936 was a caesural point in the development of the "system" of Polish policies, which aimed at the defeat of Rapallo and the Eastern Pact, the spirit of Locarno and the Pact of Four (p. 292), and whose final objective was "the mutual isolation of the Western Powers, Germany and the USSR" (p. 299).

This is a stimulating book, which clearly and logically poses and interestingly argues and analyzes the basic issues of Polish policies during the critical years of the interwar period. The study is well written, even though here and there it still shows the marks of a dissertation. It is also well documented but has no index and no bibliography.

Basically, the years 1932–33 are now accepted as the traditional caesural point in Polish historiography for the formation of Polish policies. By that time the major attitudes, assumptions, and principles guiding Polish policies were already clearly visible,as they were shaped by Marshal Józef Piłsudski. Beck, after all, was one of Piłsudski's epigones and faithfully carried out his will. One of those guiding principles was the concept of *nic o nas bez nas* (nothing about us, without us), which can serve as a key to the understanding of Polish moves all the way from Locarno in 1925 to the Munich Agreement in 1938. After Locarno the Poles refused to act as submissive clients of France, a country that by 1932 the Poles felt they could not trust but whose support was essential to the security of Poland. But, most of all, the Poles were afraid and reluctant to depend on decisions of the great powers in areas vital to the Polish interests. And here history seems to have justified their fears.

The evidence presented in this book seems to support the traditional views more than those of the author, and in this sense Zacharias's thesis does not really provide us with a clearer picture of Beck's policies; one might also argue that Zacharias distorts them in a number of places. Regardless of some of its shortcomings, however, this is an important book, and specialists of Polish and East European history should be familiar with it. It also should find its place on the shelves of all research libraries.

C. M. NOWAK
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ARSHI PIPA and SAMI REPISHTI, editors. *Studies on Kosova*. (East European Monographs, number 155.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. xiv, 279. $28.00.

Although the riots that broke out in Kosovo (Albanian, Kosova) in the spring of 1981 caught many observers by surprise, trouble between the Albanian (Kosovar) and Serb populations of the Socialist Autonomous Province of Kosovo (constitutionally associated with the Socialist Republic of Serbia) had been brewing for many years. Kosovo, the historic "cradle of Serbdom," has a population that is now almost 78 percent Albanian and two-thirds that of neighboring Albania.

In the wake of these uprisings, several academic gatherings in various parts of the world focused their attention on Kosovo. The fruit of one such meeting is *Studies on Kosova*, a collection of articles based largely on papers given at a conference at the City University of New York in November 1982 that addressed the question, "Kosova: The Present Situation and Prospects for the Future." The book is divided into two parts, "History, Language, Ethnology" and "The Economic and Political Situation," corresponding to the conference's two sessions.

Studies on Kosova consists of fifteen articles written by scholars from five Western countries. The articles vary widely in length, quality, and perspective. Some

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are based primarily on scholarly research, others more on observation and experience. Some treat the subject of Kosovo directly, others only tangentially. A few also touch on the Albanian minorities in Yugoslav Macedonia and neighboring Greece. Especially noteworthy are the following contributions: Alain Ducellier's inquiry into the genesis and failure of the Albanian state in the fourteenth and fifteenth centuries; Peter Prifti's long but well-structured essay (complemented by Adi Schnytzer's more clinical analysis) on the problems of and prospects for Kosovo's economy; overviews by Sami Repishti and Paul Shoup of the evolution of Kosovo's autonomy and changes in its constitutional status; Arshi Pipa's survey of Kosovo's history (regrettably placed toward the end of the book); and Albert Lord's selection on the battle of Kosovo as reflected in Albanian and Serbo-Croatian epic songs.

The editors have clearly sought to maintain objectivity in the preparation of this volume. Still, there is a pro-Kosovar cast to the collection as a whole. The editors themselves speak of "Serbian tutelage" in Kosovo (pp. v, ix), but far more jarring is Anton Logoreci's definition of the Yugoslav term "differentiation" in the aftermath of the 1981 riots as "the slimy official weasel word for purges" (p. 185).

Although the volume suffers from some problems common to anthologies (such as the lack of an index), the editors have done a creditable job of keeping typographical errors, incorrect word divisions, and other distractions to a minimum. Moreover, they have compiled a very useful bibliography of publications on Kosovo in Albanian, Serbo-Croatian, and various Western European languages.

This is not the definitive work on past or present problems of Kosovo, nor does it pretend to be. It is, however, a useful compendium that helps fill a gap that has existed far too long. Those interested in a different approach to the question of Kosovo's development should consult Alex N. Dragnich and Slavko Todorovich's new book, *The Saga of Kosovo: Focus on Serbian-Albanian Relationships* (1985).

JOHN D. TREADWAY
University of Richmond

ANDREI PIPPIDI. *Tradiţia politică bizantină în ţările române în secolele XVI–XVIII* [Byzantine Political Tradition in the Romanian Principalities from the Sixteenth through the Eighteenth Century]. Bucharest: Editura Academiei Republicii Socialiste România. 1983. Pp. 274. 31 L.

Andrei Pippidi, the learned and talented young Romanian historian, has produced an important study on the Byzantine legacy in the Romanian provinces in the era antedating the rise of Romanian nationalism. In a sense, Pippidi's book is both an extension and a revision of *Byzance après Byzance* (1935), the classic work of his celebrated grandfather Nicolae Iorga.

Pippidi's arguments on the essence of the Byzantine political legacy in Romanian internal affairs and state structure are essentially identical with Iorga's. He rightly stresses the continuation of the Byzantine tradition, albeit in imitative forms adapted to the "objective conditions" of the history of Wallachia and Moldavia, in the legitimation of autocratic rule by the Romanian princes, in church and state relations, and in the attempted centralization of bureaucratic governmental activities. He also tempers his judgments by pointing out deviations from the Byzantine tradition primarily through adoption, by the ruling establishments, of political theories and practices derived from other sources such as French absolutism, Russian autocracy, and Ottoman theocracy. The systematic analysis of what the author classifies as the Byzantine "ideal" versus the Romanian "reality," from the time of Stephen the Great through that of the Phanariot princes, complements and supplements Iorga's earlier work.

Where Pippidi differs from Iorga is in ascribing more specific Byzantine antecedents to the Romanians' "historic struggles" against foreign enemies for national unity and independence. Whereas the arguments related to the adoption of the principles of the "Byzantine crusade" by those engaged in the Romanian crusade against the Turks are generally limited to successful campaigns by such rulers as Stephen the Great and Michael the Brave, the presumption that anti-Ottoman actions were merely antecedents for later actions directed against other enemies of the faith—whether Orthodox or nationalist or both—is apparent throughout the work.

Whether these arguments are entirely tenable matters less than the author's masterful use of sources to make them plausible. Equally learned are the author's arguments regarding the transition from the Byzantine legacy to the modern nationalist one that, as generally known, has become an ironclad prerequisite for legitimizing all political activities, internal and external, of all rulers of twentieth-century Romania. In fact, it is important to note that a postscript, which explains the reasons for the delayed publication of the manuscript (which was completed in 1975), includes an interesting summary of the latest research on and conclusions related to the Byzantine legacy in Romanian history. The reader may be interested also in consulting works published since February 1983 that the author was unable to summarize or use in his study.

As a scholarly study written by an erudite specialist on Byzantine and Romanian history, Pippidi's

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work makes a strong case for the continuity of Byzantine traditions in Romanian history.

STEPHEN FISCHER-GALATI
University of Colorado

O. N. VILKOV and D. IA. REZUN, editors. *Istoriia gorodov sibiri dosovetskogo perioda, XVII–nachalo XX v.* [The History of Siberian Towns in the Pre-Soviet Period, from the Seventeenth to the Early Twentieth Century]. Novosibirsk: Nauka. 1977. Pp. 301. 99 k.

Notwithstanding the recent spate of books dealing with one facet or another of urban development in Russia, comparatively little literature is available on the peripheral regions of the empire as opposed to the European Russian core. This volume of essays on the history of Siberian towns, the second in an irregularly published series on this general topic, goes some way toward offsetting this imbalance.

The book is divided into four parts, the first and third of which will be of particular interest to economic and social historians. The first deals with matters related to historiographical procedure and sources. Much source material is archival in nature and, consequently, often difficult for Western historians to gain access to. Some important published primary sources, however, are available. Especially valuable for the study of urban development is the one-day census. Several of the more than one hundred censuses taken in some sixty-odd towns during the late imperial era are available for Siberia, including those for Omsk, Barnaul, Novonikolaevska, and Tomsk. These are described and assessed by L. N. Tsepliaev and B. P. Shiplov in one of the most useful papers in the first section of this collection.

The second part of the book includes three papers on general problems related to settlement, industrial development, and urban governance. The third part, however, focuses explicitly on social and economic aspects of Siberian urban development. Representative of the nine chapters in this section is that by A. D. Kolesnikov, which examines the history of Omsk in terms of the social class composition and the occupational structure of the population. The disproportionate presence of the nobility among Omsk's citizenry, which is evident from the 1897 national census, had early beginnings, as Kolesnikov observes. It owed much to two factors: first, the city's role as a garrison and, second, the possibility of living reasonably well in Omsk on a modest income or pension. From the standpoint of the urban economy, a relatively low cost of living did not by any means imply stability of employment. In Omsk's case, industry was never a major employer. Trade and commerce dominated the noninstitutional employment base, and these were much dependent on the annual harvest, distant domestic demands, and still more distant export markets. The coming of the railroad welded Omsk more fully into the national economy, but the city remained a place of volatile employment trends and general economic fragility throughout the late imperial era, as Kolesnikov's paper attests. Despite the implicit ideological underpinnings of the chapter on Omsk, it, like the eight others in this section, offers considerable grist for the Western historian's mill.

The final part of the book comprises four studies on the history of construction and architecture. The best is that of V. T. Gorbachev on Kolyvan. Vernacular architecture from various periods of the town's history gets due consideration for a change. And, indeed, the shift in focus away from institutional, commissioned architecture affords some interesting insights into the daily life and labor among the ordinary townfolk. The description of homes and outbuildings underscores the importance of livestock and kitchen gardens for the provincial town until the Great War.

Edited collections are notoriously difficult to bring off successfully. A consistent, integrative theme running through all contributions is rarely present, and in this respect the volume under review is no exception. Yet there is certainly enough here to justify more than simply a quick read. Indeed, historians will find much of value in these essays.

J. H. BATER
University of Waterloo

O. V. ORLIK. *Dekabristy i vneshniaia politika Rossii* [The Decembrists and Russian Foreign Policy]. Moscow: Nauka. 1984. Pp. 284. 1 r. 80 k.

O. V. Orlik's new book is a full-scale treatment of Russian foreign policy as seen, advocated, and to a small extent even conducted by the Decembrists (by those who held diplomatic or other relevant posts). The Decembrists in the author's definition include members of all societies and circles associated with the movement and also their "sympathizers" and "friends"—in other words, the great bulk of liberal or progressive Russia. Pushkin is mentioned on nineteen pages and Griboedov on twenty-five. The narrative moves from the West European revolutions of the 1820s "and the counter-revolutionary policy of the Holy Alliance" to the national liberation movements in the Balkans, to the Caucasus, and, finally, to Central Asia and the Far East. The last chapter deals with "the development of new principles of foreign policy" and is followed by a brief conclusion. The book is largely an expert culling of references by the Decembrists to the above-mentioned subjects.

Orlik thinks highly of the Decembrists. Although limited by their class identity and thus occasionally

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mistaken—for example, when they were insufficiently sensitive to the non-Russian nationalities of the empire—they were, on the whole, the progressive elite of their time, had the proper vision of history, and championed the right causes in foreign policy, all the way from the Pyrenees to their "correct" view of the Russo-Chinese border. As expected, the Russian government presents more of a problem to the author. Reactionary and at times imperialist, it was nevertheless closely linked to positive Russian interests, and it receives a notably kinder treatment than, for example, its British rival. Besides, subjectively imperialist acts were in some cases objectively beneficial to the peoples concerned. Moreover, these peoples, including Poles and Central Asian nationalities, generally loved Russians, even when they objected to the oppressive Russian government. Orlik never hesitates to pass judgment and to indicate who is to be endorsed and who condemned and in what measure. At times, obvious or trite views are spelled out with pomposity as precious and pioneering progressive doctrine. Although Orlik knows the Decembrists very well and orients herself easily in the Europe of the time, the only three footnotes in the conclusion are, appropriately, to Lenin.

The Decembrists did indeed contribute an important, as well as striking, chapter to Russian history, and Orlik deserves our gratitude for collecting their opinions on certain significant subjects. But her dogmatic, narrow, and predictable interpretation diminishes the value of the volume to the Western reader and, one would think, to the Soviet reader as well.

NICHOLAS V. RIASANOVSKY
University of California, Berkeley

JO ANN RUCKMAN. *The Moscow Business Elite: A Social and Cultural Portrait of Two Generations, 1840–1905*. Dekalb: Northern Illinois University Press. 1984. Pp. xiii, 275. $24.00.

In six vividly written chapters based on broad published documentation, Jo Ann Ruckman characterizes and assesses the role of about twenty families of prominent Moscow merchant-entrepreneurs in the second half of the nineteenth and the first decade of the twentieth century. First, she gives an account of the rise to wealth and prominence of these elite families—the best known among them are the Tretiakov, Morozov, Mamontov, Shchukin, Konovalov, Guchkov, and Riabushinskii clans—in the first half of the nineteenth century. As she makes clear, they usually stemmed from state or serf peasants (or village traders), which points to the need to look at the eighteenth-century peasantry from a less rigid, static, and exclusively serf-oriented perspective. They made their fortunes in the textile industry, which they virtually controlled by the middle of the nineteenth century. But Ruckman also notes that, far from restricting themselves, the merchant elite families widened the scope of their activities in the course of the nineteenth century so that some of them eventually came to control large industrial, transportation, banking, and insurance empires.

Having acquired their great wealth, the elite Moscow merchant families proceeded to spend some of it in ways that rebounded to the benefit of their own subjective needs as well as those of society. Ruckman's interesting chapters trace the patterns and evolution of this spending. The first generation of this wealthy elite spent mostly on church-connected charities; alms giving, they believed, was not only their Christian duty but also earned them rewards in the hereafter. From the 1860s to the 1880s some younger members of the elite clans began collecting art objects (mostly paintings) and playing Maecenas to artistic and cultural organizations; at the same time Muscovite merchants heavily contributed to institutions of public welfare (schools, orphanages, hospitals). Finally, at the turn of the century the offspring of merchants became creatively involved in the Silver Age of Russian culture, but not without detriment to their economic interests.

Did not these merchant elites try to play a political role as well? Were they not attracted by the example of West European and American business participation in politics? Ruckman shows that the reason for the modest and ambiguous role played by the Muscovite elite in public affairs was that these men had neither a tradition of civic activism nor much freedom of action. The situation changed somewhat in the second half of the nineteenth century. After the municipal reforms of Alexander II and Alexander III, the merchant elite came to constitute the major dominant group in the Moscow duma. At first they were passive participants, but, with the election of N. A. Alekseev to the office of mayor, they became actively involved. The energetic Alekseev was a leading reformer of Moscow's municipal services, and his example was emulated by other members of the elite families. Finally, labor problems and the revolutionary events of 1905 drove members of the Guchkov and Konovalov families to initiate steps that led to direct involvement in national politics.

Ideally suited for inclusion in college reading lists, this book nicely supplements the more arid "social science" treatments that the Moscow entrepreneurial class has been subjected to. It is a pity, however, that the publishers were not able to reproduce some portraits of the leading personages of Ruckman's story, which would have enhanced the attractiveness as well as the usefulness of the volume.

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No book is perfect, of course. My major reservations may be listed briefly. First, the nonspecialist might wish for a more elaborate discussion of the key concept *soslovie* (estate, corporate organization), the more so since, as Ruckman admits, it had special and far from uniform connotations in Russia. Second, I am not sure that generations can be as clearly distinguished as Ruckman has done: the chronological (and genealogical) boundaries are quite fluid. Finally, I am not persuaded that technical terms should be used so liberally in their Russian form; an English equivalent would make for greater readability without loss of historical accuracy.

MARC RAEFF
Columbia University

E. L. RUDNITSKAIA. *Russkaia revoliutsionnaia mysl': Demokraticheskaia pechat', 1864–1873* [Russian Revolutionary Thought: The Democratic Press, 1864–73]. Moscow: Nauka. 1984. Pp. 328. 2 r. 40 k.

E. L. Rudnitskaia has worked a narrow furrow in the well-plowed field of *narodnichestvo* (populism) where historians of the Russian revolutionary movement (from both the East and the West) have labored diligently over the past three decades. Soviet historians have explored populism as a preparation for the arrival of Marxism; historians in the West have emphasized the distinctly non-Marxist character of the indigenous Russian socialist tradition.

Rudnitskaia has remained well within the broad framework laid down by her colleagues, as she herself emphasizes in her introduction (which gives short shrift to non-Soviet scholarship on her topic). Her subject—what she calls the "democratic press" during the decade under discussion—actually consists of a handful of small newspapers and journals published in Switzerland by Russian emigres. Rudnitskaia argues that, by incubating a more revolutionary populist ideology in the period of "exhaustion" that set in after the Polish rebellion of 1863, these periodicals prepared the way for the first true socialist revolutionary activists, the young men and women who went "to the people" in 1874.

The Russian papers of Switzerland expressed the churning debate that went on in Russia itself as the revolutionary populists disputed such questions as the nature of the peasant village commune (*obshchina*), the revolutionary spirit of the peasants, the role of intellectuals, and the importance of Western precedents. They were followers of the main populist thinkers—Alexander Herzen, Nicholas Ogarev, Nicholas Chernyshevskii, Michael Bakunin, and Peter Lavrov—and of the sinister revolutionary activist Sergei Nechaev. They put out newspapers aligned with the various general currents of thought: *The Bell*, *The Present*, *The People's Business*, *Underground Word*.

Whether these papers were actually "uncensored, illegal, and democratic" (p. 5) in the full meaning of these words is questionable. They were uncensored in Switzerland but not in Russia, legal in Geneva but illegal in Moscow. As for their democratic character, they collectively represented discordant voices, but not with the aim of promoting open discussion of all points of view.

The book consists largely of exegeses from these newspapers and journals and, therefore, is heavy on theory. But that approach, relevant to Russian thought, is creakily old-fashioned with respect to newspapers and journals. Modern students of the press are no longer satisfied to study the press only on the basis of what appears in print. They want to know, for instance, about the numbers of papers printed, their distribution, and, in so far as is possible, their influence on readers. Rudnitskaia, however, sticks almost completely to summaries of articles.

CHARLES A. RUUD
University of Western Ontario

R. A. KIREEVA. *Izuchenie otechestvennoi istoriografii v dorevoliutsionnoi Rossii s serediny XIX v. do 1917 g.* [The Study of Native Historiography in Prerevolutionary Russia from the Mid-Nineteenth Century to 1917]. Moscow: Nauka. 1983. Pp. 214. 1 r. 60 k.

R. A. Kireeva is the author of a number of articles and at least one other book on prerevolutionary Russian historical scholarship, a subject that Academician Militsa Nechkina has made a thriving field of study since 1960. Nechkina has led a group of historians to concentrate on a wide variety of topics, including what she calls "the crisis in bourgeois liberal historical scholarship" at the turn of the century.

Kireeva's analysis of "the history of historical science" before the revolution, particularly from S. M. Solov'ev in the 1850s to 1910, rests on review of the works, lectures, and manuscripts of those scholars interested in the history of Russian historical scholarship: Solov'ev, K. N. Bestuzhev-Riumin, M. O. Koialovich, V. S. Ikonnikov, A. S. Lappo-Danilevskii, V. O. Kliuchevskii, D. I. Bagalei (Bahali), and even I. S. Lashniukov, who taught briefly in Kiev in the 1860s.

This is a serious, conscientious, and fair-minded volume. Kireeva's thesis is that historiography gradually became a science in the second half of the nineteenth century but that each scholar was inconsistent, that the historians disagreed with each other,

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that they concentrated on academic historians and neglected the revolutionary democrats (who wrote very little history), and that they did not comprehend the class struggle. Their national and nationalistic work, however, helped make history a science and prepared the way for the communists and true "science." Her study does help bring to the attention of its readers the qualities and views of men such as Ikonnikov and Bagalei, who, as members of the faculty in Kiev, have drawn little attention from Soviet scholars, the most able of whom are concentrated in Moscow and Leningrad and who tend to neglect the remainder of European Russia and the rest of that vast country in their works. Bestuzhev-Riumin and the most brilliant of these historians, Lappo-Danilevskii, also receive deserved attention, and Kireeva devotes twelve pages to the previously unknown Lashniukov and even two pages to N. K. Kostomarev.

The volume concentrates almost entirely on works specifically devoted to historiography and on relevant archival materials and generally neglects the authors' major works and other articles and essays. Kireeva erred seriously by limiting her approach, because the neglected sources are immensely enlightening about the events and individuals on whom the Nechkina group has concentrated, such as the Varangians, the Decembrists, "revolutionary situations," methodology, historical trends or directions, and the "crisis in bourgeois liberal historical scholarship." Clearly, the many pages that men such as Solov'ev and Kliuchevskii wrote about the Varangians and Normans incorporated information about their historiography that Kireeva should have analyzed.

Kireeva, or someone else, introduced some revisions into the text after the volume went to press, because, on one occasion, six footnotes on Miliukov were grouped into one, although the sources cited included only two encyclopedias. Other footnotes have supplementary numbers and must have been inserted after the volume went to print. The book also lacks a bibliography, an important shortcoming in a work devoted to analysis of historical scholarship and designed for professional historians, graduate students, and teachers of history.

In spite of her restrictive approach to sources, Kireeva has produced a useful book for Soviet scholars and for Westerners interested in Russian and Soviet historical scholarship.

ROBERT F. BYRNES
Indiana University

PERTTI LUNTINEN. *French Information on the Russian War Plans, 1880–1914.* (Studia Historica, number 17.) Helsinki: Suomen Historiallinen Seura. 1984. Pp. 259.

This highly specialized book looks at Russia's pre–World War I military plans and preparedness from the French perspective. Unable to gain access to the Russian military records still secreted in Soviet archives, Pertti Luntinen attempts to elaborate on A. M. Zaionchkovskii's long out-of-print *Plany voiny* (Military Plans [1926]) by using French military attaché reports deposited in the French war archives in the Castle of Vincennes. The French "information" contained in this work was chiefly gleaned and reported by three military attachés who served in St. Petersburg between 1880 and 1919, the most important being Louis Moulin, who served twenty-eight years. Unfortunately, little biographical information is given about the attachés, who remain complete nonentities; not even the full names of the two who served between 1908 and 1919, Matton and Laguiche, are mentioned. The dearth of information on the attachés' professional credentials makes it difficult to assess the accuracy and reliability of their observations.

Although the author concludes that the French succeeded rather well in discovering the plans of the general staff in St. Petersburg, he does not build a convincing case that the French influenced the Russian general staff. As it turns out, the military attachés seemed as blind to the causes of Russia's military, technological, and economic backwardness as other military specialists were. All functioned under the outdated but perfectly reasonable notion that the outcome of war would be determined by the one great battle at the opening of the war. The attachés were perspicacious, however, in informing their Paris superiors that the national minorities posed more problems for the Russian army than did the revolutionaries.

The merit of this book is that it emphasizes the complete absence among both general staffs of belief in a prolonged war. Since they firmly believed that the next war would follow the pattern of Germany's quick victories in the 1860s and 1870s, the two general staffs constantly discussed mobilization plans, railroad timetables, communication methods, and troop concentrations every time they met. They regarded as sheer lunacy the delay of one day in moving troops to the front and engaging them in battle. It is no surprise, then, that the French paid little attention to the military training and equipment of Russian forces. Likewise, the attachés attached no great significance to the social antagonisms, economic problems, and political crisis that a long war would exacerbate.

The book will primarily interest specialists of the military aspects of the Franco-Russian alliance. The author includes a series of excellent end maps showing troop concentrations and deployment, but he unfortunately does not refer to the maps in the text. Since he wishes to avoid retelling the diplo-

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matic, economic, and social background of the period, the book is episodic in nature and will be difficult for the general reader to follow. Some of this detail, such as that relating to how economic and financial relations between the two countries affected their military ties, needs to be incorporated. The Russian government refused to let the French general staff dictate the allocation of the massive French loans; as a result, the French watched helplessly as these loan monies were diverted from railroad construction to other civilian uses. Similarly, the Russian government rebuffed the French when they demanded that part of the proceeds from loans be used to purchase French railway equipment.

Although marred by editorial errors, this book enhances our knowledge of Russian war plans and preparedness.

JAMES W. LONG
Colorado State University

JAMES E. MACE. *Communism and the Dilemmas of National Liberation: National Communism in Soviet Ukraine, 1918–1933*. (Harvard Ukrainian Research Institute Monograph Series.) Cambridge, Mass.: Harvard Ukrainian Research Institute and the Ukrainian Academy of Arts and Sciences in the U.S.; distributed by Harvard University Press, Cambridge. 1983. Pp. xiv, 334. $21.50.

James E. Mace's well-written book is an exhaustive account of the Soviet Ukraine's unsuccessful attempt at "nation building" in the 1920s and early 1930s. Although Hryhory Kostiuk, Juryj Borys, Basil Dmytryshyn, George Luckyj, and Robert Sullivant have already dealt with certain aspects of the postrevolutionary decade, Mace is the first to address this turbulent period in terms of all its major political, cultural, and intellectual trends. Vivid, if somewhat less than provocative, Mace's work may become the standard one on the subject.

Mace begins by providing a broad-strokes treatment of prerevolutionary Marxist currents in the Ukraine. He continues with a discussion of the post-1917 Bolshevik's handling of the Ukrainian problem, their initial inability to adapt themselves to its constraints, and their subsequent attempt to mobilize indigenous support while retaining centralized control. The result of these developments—which, Mace could have emphasized more strongly, were not unique to the Ukrainian SSR—was *korenizatsiia*, the nationality policy counterpart of the New Economic Policy (NEP) that proved to be as "temporary" a "retreat" as the NEP.

The Ukrainian SSR's version of *korenizatsiia*—Ukrainization—is the centerpiece of Mace's book. He skillfully traces the vicissitudes of its emergence, development, and demise by focusing on Ukrainization's leading personalities: People's Commissars of Education O. Shums'kyi and M. Skrypnyk, the writer M. Khvyl'ovyi, the economist M. Volobuiev, and the historian M. Iavors'kyi. Shums'kyi, a Borot'bist turned Bolshevik, vigorously pursued Ukrainization until his disgrace in 1927 for insisting that it "be carried to the very summit of political authority" (p. 87). His critic and successor, the Old Bolshevik Skrypnyk, presided over the high point of the Ukrainian revival, desperately fought off Stalin's encroachments on the Ukraine's autonomy, and finally committed suicide in 1933. Khvyl'ovyi, an ethnic Russian turned Ukrainian, attempted to overcome the backwardness of Ukrainian literature by pressing for its break with Russia and its reorientation toward the West. He, too, took his life in 1933. Volobuiev, also an ethnic Russian, foreshadowed contemporary Third World dependency thinkers by arguing that the "legacy of colonialism in the Ukrainian language, literature, and culture could be overcome only" by a policy that recognized the Ukraine's economic autonomy (p. 175). Iavors'kyi's contribution to Ukrainian national communism was to suggest that Ukrainian history was a process distinct from that of Russia. The study ends with a discussion of Stalin's suppression of these disparate trends in the context of his onslaught against the Ukrainian peasantry and the Great Famine of 1933.

Mace's personalized approach is the source of the book's strengths and weaknesses. On the plus side, Mace demonstrates that Ukrainization was not the inevitable outcome of abstract social "forces" but a policy actively pursued and obstructed by a variety of individuals and groups. This methodology also permits him to delve into a wealth of fascinating details that might otherwise have been absent in a more analytical work. On the minus side, Mace's almost exclusive concern with discrete "superstructural" phenomena has the twofold drawback of divorcing Ukrainization from the social and economic transformation of the postrevolutionary Ukraine and of isolating Ukrainian developments from their all-union context. More important perhaps, Mace's narrative turns out to be less an analysis of "communism and the dilemmas of national liberation" in the Ukraine than a series of biographical sketches. To be sure, each individual sketch is first-rate, but their cumulative effect leaves one asking, "What does it all mean?" Had Mace been more ambitious, had he been more concerned with analyzing rather than describing, his book would have been not only informative but also definitive.

ALEXANDER J. MOTYL
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L. A. PINEGINA. *Sovetskii rabochii klass i khudozhestvennaia kul'tura, 1917–1932* [The Soviet Working Class

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and Artistic Culture, 1917–32]. Moscow: Moscow University Press. 1984. Pp. 239. 2 r. 20 k.

L. A. Pinegina has written a highly interesting and detailed account of the workers' impact on culture in Soviet Russia from 1917 to 1932. She is to be congratulated for her extensive use of primary sources, especially Soviet archival materials, which make the book a valuable addition to our knowledge of culture in Soviet Russia during the 1920s. Furthermore, it is a pleasure to see the notes placed at the bottom of each page instead of at the end of the text at the back of the work, which, unfortunately, often occurs in contemporary scholarly monographs.

Pinegina's principal theme is that traditional interpretations of the Soviet 1920s give too much emphasis to the intelligentsia—the cultural, educational, and intellectual elites—in the development of literature and the arts and neglect the contributions of proletarian artistic organizations, such as Proletcult, the All-Union Association of Proletarian Writers, and the Association of Artists of Revolutionary Russia. She does not deny the cultural achievements of the intelligentsia during the 1920s but indicates that workers' artistic organizations have not received the scholarly attention they deserve.

Her book consists of an introduction and five chapters, each of which focuses on specific proletarian cultural organizations: chapter 1 analyzes the relationship between the cultural heritage of Western civilization and proletarian culture; chapter 2 examines Proletcult, an organization that still needs more scholarly scrutiny; chapter 3 inspects proletarian literary associations, circles, and clubs; chapter 4, probably the most enlightening section of the book, concentrates on the proletariat and the theater; and chapter 5 describes music and the fine arts in the context of proletarian cultural organizations.

One of the strengths of Pinegina's work is her detailed knowledge of the subject under discussion. The Soviet 1920s witnessed the proliferation of artistic groups and organizations, each of which was determined to play a role in the creation of culture, even though some of them were in existence only for a brief period. As a result, at times her book makes for some dense reading, but on the whole she treads her way carefully through complex and difficult material.

Pinegina is certainly correct in arguing that workers' artistic organizations had a significant impact on the development of culture in Soviet Russia during the 1920s, but she might have examined more thoroughly the relations between such groups and the old intelligentsia that the Soviet government inherited from the tsarist regime. In addition, the book suffers from the lack of an index, which would be quite helpful given the amount of detail the author provides. But these are minor criticisms and detract only slightly from the high quality of Pinegina's accomplishment.

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V. M. SELUNSKAIA *et al.*, editors. *Izmeneniia sotsial'noi struktury sovetskogo obshchestva, 1921–seredina 30-kh godov* [Changes in the Social Structure of Soviet Society from 1921 to the Mid-1930s]. Moscow: Mysl'. 1979. Pp. 342. 1 r. 70 k.

A review copy of this work was slow to arrive at the *AHR* offices, yet, because of its subject matter and limited availability, the book merits notice, albeit belated. As the introduction points out, the social structure of Soviet society in the 1920s and 1930s has not yet been adequately investigated by historians. The more's the pity, for this was a dynamic period of sweeping social transformation that laid the foundations of contemporary Soviet society.

This is the second part of a collectively authored two-volume study of postrevolutionary Soviet society. A synthetic work, it draws on the results of previous, more specialized studies rather than offering new findings or interpretations. The first volume, published in 1976 under the same title and also edited by V. M. Selunskaia *et al.*, covers the years 1917–20 and opens with chapters on Marxist-Leninist ideology, historiography, and sources, to which readers are referred in the present volume. Seven of the sixteen established scholars contributing to this volume contributed also to the first. The end product of the collaboration is more coherent than might be expected, but frequent shifts in style and treatment make for a bumpy read.

The work is divided into two sections—one dealing with the period from 1921 to the end of 1929, the other continuing into 1936–37 (and sometimes later). Each part has a chapter on the workers, the countryside, and the intelligentsia. The first section includes also a chapter on urban capitalists, and the second is introduced by one on the "role of the Soviet state in the planned regulation of social processes."

A good deal of information can be found here, much of it statistical, on such questions as the size and composition of social groups, the movement of literacy rates, changes in the female work force, and differences among the union republics. One of the most arresting points touched on is the age profile of this malleable society. In 1926, on the eve of the great transformations (planning, industrialization, collectivization), half of the entire population was in the 18–29 age bracket; only 7 percent was over 50

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years old. The weight of this factor in lowering social resistance to change invites closer examination.

Some of the statistics, however, invite another sort of scrutiny. For example, Lenin's estimate that the number of workers in 1913 was "probably about 20 million" is quoted in volume 1 (p. 127) and, after some tergiversation, firmly endorsed. Yet the present volume states that the number of workers and government employees in the national economy in 1913 was 11.4 million (p. 23). What is one to make of references to the size of the work force in later years when it is given as a percentage of the "prewar level"?

The chapters on the "intelligentsia" (here, an ill-defined catch-all category for old and new professionals and technicians, including the military) barely begin to open a tangled topic. The flashfire spread of education and the melting of old barriers to social mobility emerge from the bare data, but one is reminded that the book purports to deal with social structure, not with the content of the structure. A list of cooperative "bourgeois" historians described (p. 285) as working in Soviet institutes in the 1930s comes as a surprise. Of the six scholars named, A. N. Savin died in 1923, M. M. Bogoslovskii and A. E. Presniakov in 1929.

If the book tends at times to be regrettably formularistic, it also offers passages of measured discretion and scholarly balance. The work provides a useful summary of fascinating and complex social processes, but perhaps its chief value lies in its revelation, often unwitting, of the many issues that remain to be explored in this period of incessant social turmoil, triumphs, and tragedies.

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A. S. MOSKOVSKII and V. A. ISUPOV. *Formirovanie gorodskogo naseleniia Sibiri, 1926–1939 gg.* [The Formation of the Urban Population of Siberia, 1926–39]. Novosibirsk: Nauka. 1984. Pp. 167. 1 r. 60 k.

This book is perhaps most important for its introduction. It is also an indication that Soviet officials are conscious of severe demographic problems in the USSR. A. S. Moskovskii is prolific. His books reexamine similar topics from slightly different viewpoints. He has published books on the growth of the cultural level of Siberian workers, on the workers of Siberia, on the industrial development of Siberia "in the period of the construction of socialism," and on "the growth of labor activity of the working class of Siberia during the construction of socialism." V. A. Isupov seems to be his protégé.

In chapter 1 the authors justify their work by the mandatory quotations from Lenin, as well as by others from Marx and Engels, before discussing the existing literature on the subject and why their work is needed. Having also discussed their sources—basically census data—the authors then define terms they will use such as "organized recruiting" and "mechanical increase." This would seem standard for such a work, as is the treatment of "urban contingents" at various times, the sources of this population in terms of class, sex, occupation and area, and changes in the population through time. The authors provide much statistical data in the text and in sixty-two tables. Not surprisingly, they report that cities grew in Siberia owing both to natural increase and to migration from the countryside and from European Russia. Many figures are given to explain how this occurred, and the citations require fifteen pages. There is an index.

The work, then, appears to be a solid piece of research. But there are serious problems. Nowhere do the authors mention the corrective labor camp system. They do not explain whether such vast collections of camps and prisoners as those involved in the construction of the giant Bratsk hydroelectric plant are considered urban areas (and included) or not. What happened to former prisoners legally required to live within five kilometers of their last place of confinement? Even granting that the death rate in the Stalinist camp system was very high, it was not 100 percent, and we are given no information on what must have been many tens of thousands of such former prisoners.

Since Moskovskii and Isupov have not explained how these questions were handled, it is difficult to see just how the book can be generally useful. All the tales and figures must remain suspect. But to experts, armed with information from other sources, this book may be of great value, provided they remain aware of its limitations. The introduction, however, is significant for everyone. There, the authors point out that the Twenty-sixth Party Congress called for an effective demographic policy, and they argue that such a policy cannot be well founded without studying the history of population changes. In other words, they argue that party policies should be based on history. In the Soviet Union under Stalin, Khrushchev, and Brezhnev, history was always the product of party policy, not its source. That Soviet authorities have published an argument to reverse this policy and use history to determine new policies is a hopeful sign.

PATRICK R. TAYLOR
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L. A. DERBOV. *Istoricheskaia nauka v Saratovskom universitete* [History at Saratov University]. Saratov: Saratov University Press. 1983. Pp. 149. 1 r.

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Since the early 1960s the history faculty (*fakul'tet*) of Saratov University has emerged as one of the most vital and professionally competent ones outside Leningrad and Moscow. Perhaps the most telling evidence of the department's growing prominence is the appearance during this period of six new historical journals and irregularly published collections of scholarly papers: *Antichnyi mir i arkheologiia* (The Ancient World and Archaeology), *Srednevekovyi gorod* (The Medieval City), *Novaia i noveishaia istoriia* (Modern and Contemporary History), *Osvoboditel'noe dvizhenie v Rossii* (The Liberation Movement in Russia), *Istoriograficheskii sbornik* (Historiographical Collection), and *Povolzhskii krai* (The Volga Region). To place the present expansion of scholarly activity in historical perspective is the aim of this modest book under review.

L. A. Derbov, who has taught history at Saratov University for over forty years, designed his monograph for students of the history faculty, especially for those enrolled in a special course on the historiography of the history of the USSR. Such courses and published surveys are part of a long-overdue national effort to illuminate the rise of the Soviet historical profession (see, for example, Iu. S. Kukushkin *et al.*, eds., *Istoricheskaia nauka v Moskovskom universitete, 1934–1984* [1984]). As can be expected, the author offers the reader a sanitized chronicle of achievements, occasionally punctuated by an oblique reference to human shortcomings. It is the stuff of college catalogues and promotional brochures without reference to the extracurricular activities. In other words, it makes for pretty dull reading.

Nonetheless, Derbov's efforts are not without interest, if for no other reason than we know so little about the vicissitudes of the development of the discipline of history in the Soviet Union. Moreover, the history program at Saratov opened in the fateful year 1917 and is therefore inseparably linked to the Soviet regime itself. (Since enrolling its first students, the Saratov faculty has trained more than five thousand graduates.) In six chapters Derbov traces the history of the faculty, describes the activities of its five departments (*kafedry*), and assesses their research activities over the years. Appendixes provide useful biographical and bibliographical information on leading historians. I found Derbov's treatment of the pre-1945 period the most interesting part of the book, both for what it includes and for what it ignores. A surprising level of research activity got launched during the tortuous years of the Civil War, only to succumb to the disruption of the profession and higher education in general during the First Five-Year Plan. An important party circular of May 1934 saved the universities from ruin and reestablished history faculties at Moscow and Leningrad universities. In 1935 Saratov University opened the first history faculty in the provinces. During this formative period influential historians such as A. M. Pankratova, N. L. Rubinshtein, V. V. Mavrodin, and P. G. Liubomirov taught in Saratov, some by choice, others out of necessity.

Judging by present trends, provincial history faculties such as those in Saratov, Tomsk, and elsewhere could come to play an increasingly important role in training future generations of Soviet historians. Yet, despite the accomplishments and visibility of several key programs, the faculty at Saratov lacks personnel and library resources to become truly first-rate. Even if it succeeds in improving on some of the shortcomings Derbov admits, it still will be unable to train graduate students in Asian, African, and American history. Furthermore, and perhaps most serious of all, the department has few ties with the outside world. Until its members are permitted to develop regular, sustained contact with foreign historians, especially with those of us of bourgeois capitalist persuasion, it will never become more than a good provincial program with a few areas of fortuitous excellence.

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JAMES M. SWANSON. *Scientific Discoveries and Soviet Law: A Sociohistorical Analysis*. (University of Florida Monographs, Social Sciences, number 70.) Gainesville: University Presses of Florida. 1984. Pp. viii, 150. $11.00.

This book describes the history and operation of the formal mechanism provided by Soviet law for the legal recognition of basic scientific discoveries (as opposed to patentable technical inventions). James M. Swanson draws both on extensive Soviet source materials and on the standard Western studies on the sociology of scientific rewards to provide a great deal of information on the nature of debate within the Soviet Union on the scientific reward system. The work starts with a history of the various unsuccessful efforts to protect discoveries in international intellectual property law and then continues with a chronicle of Soviet legislation. Further chapters discuss the Soviet theory and practice of protecting discoveries, Soviet attempts to win international recognition for the protection of discoveries, and the place of discoveries in the "normative structure" of Soviet science.

The book raises the fundamental science policy question: should there be a formal legal mechanism for the protection of discoveries? This question leads to two more: does the discovery protection scheme play a useful role in Soviet science; should other countries consider adopting such a scheme? Swanson tries to keep these questions open, but the

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history he skillfully recounts suggests a negative answer to both of them. Soviet authorities have been flooded with applications for "discoveries" of little or no scientific value and have reacted by rejecting 99 percent of the applications. A disproportionate number of awards go to established, senior Soviet scientists and too few to promising young scientists. The total number and average size of awards are too small to have any significant influence. Leading Soviet scholars have suggested remedies ranging from abolishing the system to increasing the cash payments substantially. Indeed, the author's account of the Soviet debate on these issues is the most interesting part of the book. The failure of Soviet attempts to secure international legal recognition for discoveries suggests that there is no perceived need for such a system in countries (like the United States) where the informal methods for rewarding scientific discoveries are much more developed than in the Soviet Union.

Readers without a background in patent law may have some difficulty in understanding the dividing line between inventions and discoveries, a dividing line that is crucial to the understanding of the debate on protection of discoveries. From the examples of discoveries cited by the author, it would appear that, in the USSR, the scope of inventions is narrower and the scope of discoveries broader than in most other countries, including the United States. A clever patent attorney could easily obtain patent protection in many countries for practical applications of many of the discoveries listed. Obviously, the broader availability of patent protection lessens the need for special discovery protection.

This monograph provides a clear image of one small aspect of the complex institutional and human structures of Soviet science. With time, the accumulation of such detailed studies can provide outsiders with a much better idea of how Soviet science works.

PETER B. MAGGS
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KAREN DAWISHA. *The Kremlin and the Prague Spring.* (International Crisis Behavior Series, number 4.) Berkeley and Los Angeles: University of California

When does a Sovietologist become a historian? First, when the topic covered is clearly bounded by time limits, is far enough in the past, and has available a body of hard evidence from non-Soviet as well as Soviet sources against which to check the nebulous deductions and speculations of customary Sovietology. Second, when the investigator has the industry to explore for every scrap of pertinent material, the patience to piece together the story of how and by whom key decisions were made, and the ability to recognize the gaps and blank spots in the story while still not fearing to illuminate them by applying knowledge of the Soviet system and, if you will, the arts of the Sovietologist. Soviet reactions to the Prague Spring and the Kremlin's decision to intervene fit the above description of such a historical topic, and Karen Dawisha is such an investigator and author. This is a first-rate book, one not likely to be superseded unless by the improbable work of some improbable individual with access to the minutes of the Politburo.

The subject itself is not new to scholars. Jiri Valenta wrote an excellent monograph on it a few years ago (*Soviet Intervention in Czechoslovakia, 1968* [1979]). He and Dawisha do not use widely differing methods or reach conflicting conclusions. Both picture the Soviet leadership as divided and uncertain about how to deal with Prague's revisionism until very late in the game, although they do differ on when the vital decision to prepare military action was taken. Both picture Kosygin and Suslov as doves, Shelest and Podgorny as hawks, and Brezhnev as the man in the middle who finally saw no alternative to intervention.

Where Dawisha excels is in her thoroughness; she makes the most of the limited Soviet material and also uses the many facts, observations, and revelations available from the Czechoslovak side, from Western diplomats and intelligence officers (including hitherto classified U.S. documents), and from interviews with participants. The author stresses, rightly, the intense Soviet sensitivity to the effects of the events in Czechoslovakia on the rest of Eastern Europe; we see Ulbricht and Gomulka pressing for strong action against Prague and being heeded; Kádár trying to play the conciliator but in the end having to fall into line; Ceauşescu and Tito, bad examples, distrusted by Moscow and excluded from consultation and decisions. She also documents how Soviet domestic concerns, mainly the fear of ideological infection, created constituencies in the Politburo for strong action in Czechoslovakia.

The book has another dimension. Sovietology serves political science as well as history. A colleague of Michael Brecher and a participant in his International Crisis Behavior Project, Dawisha attempts to test Brecher's various hypotheses and to apply some of the rules and criteria—such as the timing of phases of precrisis and crisis, the effects of stress on the decision makers, the narrowing of alternatives as the affair proceeds—drawn from other models and other crises. The exercise has the merit of focusing the attention of author and reader on all the relevant decisions (listed here by number and date) and the reasons for them, but much of the argument seems in the nature of a bow to the theorists. It is not easy to demonstrate that Soviet foreign policy mak-

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ing, where at best our knowledge is fragmentary, conforms to any general model.

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NEAR EAST

MAJID KHADDURI. *The Islamic Conception of Justice.* Baltimore: Johns Hopkins University Press. 1984. Pp. xvi, 256.

Majid Khadduri is a well-known authority on Islamic law, and the Islamic conception of justice is an enticing subject about which we have little concrete knowledge. It is thus with pleasure that one picks up this book, which is, moreover, nicely produced. But it is with altogether different feelings that one puts it down again some two hundred forty pages later, for Khadduri has nothing of substance, let alone anything new, to say on the subject.

Contrary to what one might expect, the book is not devoted specifically to Islamic views on legal justice but rather to Islamic views on justice in its legal, social, political, theological, philosophical, and ethical senses between ca. 700 and today. The subject matter is thus somewhat diffuse, all the more so in that the book lacks an overarching theory and frequently discusses topics largely or wholly irrelevant to justice in the various senses proposed. Thus, "political justice" turns out to consist of little more than the well-known Sunnī, Khārijite, and Shīʿite theories of the imamate, and "social justice" unexpectedly takes us (*inter alia*) to Ibn Khaldūn's famous theory of history, followed by some observations to the effect that Ibn Khaldūn came to regard the enforcement of Islamic law as essential for the prevention of moral decline. "Justice between nations" is simply the Islamic law of holy war (or "just war" as Khadduri calls it). Even the chapter on legal justice consists largely of commonplace observations on general welfare, happiness, good faith, and the like. On jurisprudential theories of justice there is not a word.

As an introduction to the various subjects covered, the book is of little value. Khadduri consistently fails to refer the reader (and at times himself) to the works of the foremost authorities on the subjects in question. Thus, he writes of Murji'ism, Zaydism, Ismāʿīlism, *ahl al-sunna*, the created Qur'an, and the like without reference to W. Madelung; on mirrors for princes without reference to A. K. S. Lambton; on Miskawayh without reference to M. Arkoun; on Mawardi without reference to H. A. R. Gibb; on law without reference to J. Schacht; and on Afghānī without reference to Nikki Keddie and Elie Kedourie. In addition, the presentation is often misleading, off target, and simplistic. Sunnism was born not at the time of the Prophet (as implied on page 20 and elsewhere) but in the ninth century or later, depending on one's definition of the phenomenon. And how is one to envisage a Sunnī Qadarite of the Umayyad period (p. 30)? Jabrism was not the "official doctrine" of the Umayyad court (p. 25 and elsewhere; a crude version of a current if implausible theory); the Murji'ites did not directly or indirectly support the Umayyad regime (p. 27 and elsewhere); and Shāfiʿī's theory of *ijmāʿ* had no bearing on the choice of sovereign (p. 18; if Khadduri has seen it invoked in this context, documentation is needed). Numerous Arabic terms are incorrecly transliterated: "contract" is *ʿaqd*, not *ʿiqd* (p. 88), and so on.

What is so disappointing about this book, however, is not so much its inaccuracies as its sheer vacuity: Khadduri says extremely little and in so bland a fashion that it is sometimes difficult to recognize the familiar contours of what is being described. Its general message is that the Islamic conception of justice was admirable, and so no doubt it was. But whatever else Islam may have been, it was (and is) a culture with well-defined features of its own, not, as Khadduri occasionally seems to suggest, a vague attempt to say things of the kind we like to hear in the Western world today. *Pace* Rouhollah Ramazani, the book is not of interest to students of either Islam or law, let alone to "general theorists" (p. x). In so far as its purpose is to evoke admiration for Islam, it fails in that respect too: the portrait is simply too faceless.

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ULRICH FIEDLER. *Der Bedeutungswandel der Hedschasbahn: Eine historisch-geographische Untersuchung.* (Islamkundliche Untersuchungen, number 94.) Berlin: Klaus Schwarz. 1984. Pp. ix, 338.

The first half of Ulrich Fiedler's book on the Hijaz Railway deals with its historical background, and the second half covers recent, contemporary, and future aspects of the railway. In the historical section Fiedler provides a survey of railway construction in the Ottoman empire and a more detailed look at foreign-built railways in the Syrian region.

The Hijaz Railway was an exception to the general pattern of railways in Asia and Africa: it was financed, built, and operated by the Ottomans themselves, not by a European power. It was also unusual in that it had a religious purpose—facilitating the pilgrimage from Damascus to Medina and Mecca—as well as strategic and economic goals. Fiedler traces the planning, administration, financing, operations, and construction of the rail-

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way. He disagrees with earlier studies of the railway, among others, in the two following ways: he sees the financing of the railway as an exception to Ottoman weakness, and he interprets the Taba crisis of 1906 between the Ottomans and the British as being caused chiefly by long-term uncertainty over the border rather than the Hijaz Railway's planned branch to Aqaba and Taba.

The military value of the railway to the Ottomans became clear during World War I, when soliders of the Arab Revolt, led by Faisal ibn Husain and advised by T. E. Lawrence, attacked and damaged much of it. After the war the railway ultimately fell under the control of the Palestine, Jordan, and Syrian mandates and the Saudi Kingdom. From 1928 to the present Jordan, Syria, and Saudi Arabia have been discussing, with varying degrees of enthusiasm, rebuilding the southern sections of the railway and its integration and operations. The oil boom of the 1970s and Jordan's opening of a branch line to Aqaba for the export of phosphates in 1975 have accentuated the potential value of rebuilding the Hijaz Railway, a project Fiedler believes would be beneficial.

In writing the historical section of this book the author has diligently and thoroughly used German, Austrian, British, and French archives and periodicals, as well as a multitude of other materials. The chief value of the book lies in the information he draws from the German and Austrian archives and new insights based on these sources. The Ottoman archives, which may well contain a wealth of pertinent information, were not consulted, however. Also, David Long's *The Hajj Today* (1979) is missing from the bibliography.

By drawing together the whole history of the Hijaz Railway, including the period since World War II, the author has enabled those interested in social and economic history to examine a most unusual and tangible legacy of the Ottoman empire to the modern Middle East.

WILLIAM OCHSENWALD
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ISRAEL STATE ARCHIVES. *Documents on the Foreign Policy of Israel: Political and Diplomatic Documents, December 1947–May 1948*. Jerusalem: Government Printer. 1979. Pp. xl, 888.

ISRAEL STATE ARCHIVES. *Documents on the Foreign Policy of Israel*. Volume 1, *15 May–30 September 1948*; volume 2, *October 1948–April 1949*; volume 3, *December 1948–July 1949: Armistice Negotiations with the Arab States*. With *Companion Volumes*. Jerusalem: Government Printer. 1981–84. Pp. xlviii, 711; lxvi, 251; lxxv, 229.

Documents on the Foreign Policy of Israel (DFPI), which focuses on the planning, formulation, and execution of foreign policy, is a new series that the State Archives in the Prime Minister's Office began publishing in 1979. Over the first five years the editors, with customary Israeli zeal, have produced four volumes (seven, if the companion volumes are counted) to document the unfolding diplomacy of the new state in the first twenty months, including the state's precarious six-month gestation. The five-year span offers a sound basis for evaluating the series. Such reference works, as a rule, attract mostly diplomats and researchers. DFPI probably also lures buffs of Israel, of the Palestine resistance movement, and of the Arab-Israeli dispute who seldom interrupt the search for new evidence to confirm old prejudices.

The series editors were clearly inspired by *Foreign Relations of the United States* (FRUS), which the Department of State launched in 1861. It was an annual publication—with occasional special sets on multilateral conferences arising from the two world wars—until 1984, when it became a triennial. Israel's archivists were also influenced by Britain's thirty-year declassification rule, introduced in 1968. Unlike the Public Record Office in London, which places cabinet minutes and papers on open shelves (with minor exceptions released on schedule), the State Archives in Jerusalem withheld the parallel cabinet documents. The Israeli series informs policy making, motives, and diplomatic style and discloses precise authorship.

The external concerns of the "average" small state, it is commonly assumed, are circumscribed almost wholly by its immediate neighborhood. Until the energy crisis of the 1970s thrust OPEC members into the role of global actors, Israel was probably unique among small states in the ramified, earth-girding relations that it cultivated. Even at birth it was already interacting with the industrial West (chiefly the U.S., Britain, and the British Commonwealth), communist Eastern Europe, and many Arab and Islamic countries. Israel's global diplomatic reach was attributable to the Jewish diaspora on which, in the critical formative years, it leaned heavily for immigration and security and the material resources to sustain both.

Political and Diplomatic Documents, December 1947–May 1948, the first volume of the series, was the joint venture of the State Archives and the Central Zionist Archives. Retrospectively labeled the *Preliminary Volume*, it documented the transition from the Jewish quasi-government of the mandate to the provisional government that proclaimed Israel's independence. Nation and state building among the Palestine Jews had made solid progress under British rule. The abrupt transition to sovereignty, once partition had been decided, thus

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proved institutionally—though not politically—less traumatic than for almost any other postwar liberation movement. Most presovereign entries came from the Central Zionist Archives, still the depository for such records.

Roughly one-fourth of the *Preliminary Volume*—the front matter, many documents, the notes, and the index—is in Hebrew, thus diminishing its value for many. This disadvantage has since been compensated for. Each main postindependence volume in the described form has a *Companion Volume* that gives summaries of the Hebrew documents but translates in full the front matter (an analytical essay on the major themes and a list of the documents), the annotation, the editorial notes, and a combined index to the linked volumes. Though cumbersome, the method opens the series to users who know no Hebrew. Cross-references to FRUS and to the UN *Official Records* enable the reader easily to locate such related diplomatic papers in print. A full translation of the Hebrew documents, rather than summaries, would further enhance the DFPI's worth without adding greatly to its length.

The first four volumes address the diplomatic issues attending the creation of the Provisional Government of Israel (PGI), its evolution into a permanent regime, and the progressive legitimation of the new state leading to its admission to UN membership. Also treated are the conduct of the civil war and the regional war into which it expanded at the mandate's end, as well as the stages by which the UN Security Council and General Assembly contained and eventually stopped the fighting and, with peace still elusive, helped the parties create an armistice system.

Pragmatic purposiveness shines through the record of the first fourteen months of sovereignty. The Foreign Ministry tenaciously pursued recognition of the PGI, mainly in the British Commonwealth and across Europe. In these countries, it used the inherited Jewish Agency offices that grew into consular and diplomatic missions the moment a host government recognized the PGI. Only the USSR gave Israel de jure recognition at once, so the Foreign Ministry mounted a simultaneous campaign for changing each de facto recognition into a de jure one. The foreign minister and the PGI representative at the UN coordinated yet another campaign for Israel's admission to UN membership. The hectic process, followed in detail in these documents, reveals a sophisticated diplomatic network with easy communication among its formal and informal missions in widely strewn capitals.

Despite the broad-gauged substantive coverage, the DFPI must be seen for what it is: the record of Israeli diplomacy as kept, before independence, by the political department of the Jewish Agency and, after it, by the Ministry of Foreign Affairs. The one exception so far is volume 3, devoted to the 1949 armistice negotiations in which, on the Israeli side, the Ministry of Defense also participated; the entries thus integrate military and diplomatic documents. Wholly absent from the volumes in review are the pertinent cabinet papers to reveal the role of the Prime Minister's Office and of the other ministries, chiefly that of finance, in framing Israel's foreign policy.

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AFRICA

D. T. NIANE, editor. *UNESCO General History of Africa*. Volume 4, *Africa from the Twelfth to the Sixteenth Century*. Berkeley and Los Angeles: University of California Press or Heinemann, London or UNESCO, Paris. 1984. Pp. xxvii, 751. $25.00.

UNESCO has come out with the long-overdue fourth volume of its chronological *General History of Africa*. Working more or less within the period from the twelfth to the sixteenth century, twenty-six scholars contribute twenty-seven chapters under the editorship of D. T. Niane of Senegal. Ten of the contributors are from French-speaking Africa and Madagascar; five from France (UNESCO's host country), two each from Czechoslovakia and the United Kingdom, and individuals from Kenya, Nigeria, Belgium, the United States, the Soviet Union, and the German Federal Republic. With his introduction, conclusion, and a chapter devoted to the regional intra-African connections and exchanges, the editor has made a fair effort to enhance the volume's unity.

Long-time students of African history will find few surprises in the text. Yet the volume should be welcome for everyone else and especially for readers in and out of academia who can only relate to Africa through the filter of European culture. It ends with the beginnings of the Western European entry into non-Mediterranean Africa more than a century before the start of the Atlantic slave trade. It details the major events, the subtle and forceful changes that owed little or nothing at all to the intrusions of Europe, and a past rich in originality, complex structures, and both written and unwritten sources.

Assisted by increasing exports of gold, salt, kola, and special fabrics, both city-states and kingdoms emerged among the Akan, who finally settled in what is today central and southern Ghana. An even greater success story with trade as its pivot involves the Hausa of today's northern Nigeria. Through a system of trading networks their language and culture came to dominate the central Sudan's savannah

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belt. The Hausa civilization intermingled with Islamic currents from the Maghreb and Chad, but political unity eluded them throughout the centuries under study and beyond. In contrast, the Mossi of the upper Voltaic region established strong states through a combination of agricultural expansion and military muscle without any great debts to trade. Under the authority of the *naabas*, the Mossi developed a political and religious structure that acted as a shield against foreign influence until much later. Halfway across the continent, in the Great Lakes' regions, Nilotic pastoralists moving south and the speakers of Bantu languages moving north came together and intermeshed in myriad conjugations of culture, political systems, and economic life that play havoc with theories of causality. The Afro-Muslim Swahili civilization evolved along coastal East Africa crossing into the adjacent islands, including the Comoros and northwestern Madagascar. Madagascar itself had no great centers of power and authority as yet, but the arrivals of settlers from overseas were coming to end by the early decades of the sixteenth century.

By the 1400s strong Christian states that stood for centuries in Nubia (today's northern Sudan) disappeared having succumbed to the human and animal migrations that neither the arrow nor the bullet could arrest. While the great cathedral at Faras (Nubia) fell into ruin, an Ethiopian monarch carved ten churches out of solid rock at Lalibela almost as a statement that the Ethiopian Christian empire would never be destroyed. Under the Almohads, who represent a reformist movement from within, the long and fertile interchange between Andalusia and the Maghreb ended with the *reconquista*. At the same time, pilgrims from western Sudan (including some of its wealthiest and most powerful men) flocked to Cairo, whose own merchants and military advisers were starting to go to the western Sudan. Mali and Songhay in West Africa, both arching over the great bend of the Niger, became rather large trading zones and empires applying both direct and indirect rule, with important towns jutting way out into the Sahara. Rock-salt mines and the gold-bearing southern regions affected the politics of this vast area, and Songhay became Mali's successor as long-distance trade routes shifted eastward. In what is today Zimbabwe, an almost endless supply of gold, most of it going across the western Indian Ocean, was a key factor in the advent of Mwene Mutapa's empire. Another imperial impulse in the making was the gradual coming together of two important states around Lake Chad, Kanem and Bornu, where state building and Islam appear to have had close ties. This great and intrusive religion had nothing to do, however, with state building among the Yoruba of western Nigeria centered at Old Oyo under the *alafins*, among the Luba of the Lualaba lakes, and among the groups within the strong and prosperous Congo Kingdom under the *manis*.

Cultural history receives considerable and much-deserved attention, and the volume's contributors discuss for the most part the nature and the problems of disparate evidence while presenting alternative hypotheses. This is not to say that specialists could not find areas of disagreement. For example, the map of Madagascar showing various types of migrations (p. 598) is very misleading because alternative evidence of long standing and considerable strength is not taken into account. The reason is that an ahistorical and a historically useless text have been too heavily relied on. One could also note that seventy pages of the volume have been devoted to Mali and less than seventeen to Madagascar, which is a small continent in its own right.

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JEFF CRISP. *The Story of an African Working Class: Ghanaian Miners' Struggles, 1870–1980*. London: Zed; distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. xviii, 200. Cloth $26.25, paper $10.25.

A number of excellent studies of the culture and political agency of African working-class communities have been published during the last decade, most notably, those by Richard Jeffries, Charles van Onselen, and Bill Freund. Some of the best have been concerned with mining communities, particularly in Central and South Africa. Jeff Crisp had the excellent idea of supplementing these studies with an analysis of the vicissitudes of the mining work force in Ghana over the last century and a quarter.

He deals with a number of interesting and important questions: the initial formation of this work force and the role played by the colonial administration in its recruitment and subsequent policing; the comparative weakness of mining interests in Ghana, even in the gold sector, in contrast with their role in Southern Africa; the divisions of interest and disparities in residential commitment between different sections of the labor force. Crisp's treatment of some of these issues is marred by neglect of other aspects of Ghana's history, both before and during the period he analyzes. Even the account of labor supply, which is on the whole illuminating, is weakened by the absence of any consideration of the economic, social, and political role of gold production in the precolonial history of the area, and of Asante most particularly. The course of worker resistance in the mining communities themselves is never set within a coherent framework of the economic history of the industry and its world market

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price competitiveness. Even the somewhat denser treatment of the complicated political conflicts between different segments of the work force, the mining union, the foreign mining companies, and the colonial or national government is weakened by the absence of any systematic account of the interplay between the history of governmental policy and the national economy.

Crisp is at his best when densely immersed in the history of mining communities themselves, exploring the difficult politics of solidarity and dissension in a union struggling to hold a membership whose own interests were often deeply divided. He is at his least persuasive when wrestling to subsume his subject matter as a whole into the somewhat wooden categories of a contest between mining capital and the workers whom it exploits. This categorization becomes especially infelicitous after the point at which the Ghanaian government took the markedly unprofitable majority of the mines into public ownership in order to guarantee the continued employment of most of their work force.

Crisp's is (quite appropriately) an angrily partisan picture of the place of the mining industry in the history of modern Ghana. It might have been of greater value if it had been less doggedly and rigorously Marxist and made a more adventurous and sensitive attempt to capture and convey the consciousness of his beleaguered workers. As it is, readers are left with ample assurance that Crisp's own heart is in the right place. (There can be few foreign historians of Africa whose hearts lie unequivocally with international mining capital or with expatriate white mining personnel.) But, in contrast with the ethnographic vividness of the work of Jeffries or van Onselen on other African working-class communities, Crisp gives remarkably little sense of what the practical lives and the political attitudes of Ghanaian miners in this century have, in fact, been like.

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WYATT MACGAFFEY. *Modern Kongo Prophets: Religion in a Plural Society*. (African Systems of Thought.) Bloomington: Indiana University Press. 1983. Pp. xiii, 285. Cloth $22.50, paper $15.00.

The rise and development of Kimbanguism among the BaKongo of lower Zaire has been one of the most interesting and well-studied forms of prophetism in twentieth-century Africa. The literature both on Simon Kimbangu and on the wider prophetic movement since his arrest in 1921 has been vast if of uneven quality. Yet major gaps still need to be filled.

Wyatt MacGaffey has done much to interpret Congo religion and society, as is clear from his many stimulating publications. In too many cases a book on African religion by an outside scholar is a first contribution based on relatively shallow experience. *Modern Kongo Prophets*, however, derives from the author's involvement in Zaire over some twenty years, and it has rich data and interpretive depth. No one seriously concerned with African prophetism, or indeed religious prophetism elsewhere, can afford to ignore this book, which is a sophisticated analysis of one group of prophets and little churches, as well as of dreams, symbols, and healing techniques.

MacGaffey analyzes the modern Congo predicament as one in which two worlds—the traditional and the bureaucratic—have come wholly to overlap, resulting in ongoing strain and misunderstanding. Each possess its own rhetoric. The reality of contemporary existence is inevitably removed from the norms of tradition, but, as the bureaucratic order derived from colonialism has functioned less and less well, its rhetoric has moved almost equally far from existent reality. In due course the two rhetorics become almost one in a spiritual and verbal chaos symbolized by various grotesque formulations such as the "Occult Minister of the Committee for News Service in Traditional Language" (p. 240). The strains engendered generally in society are expressed, MacGaffey argues, through heightened form in dreams and frequently bizarre ministries of "prophets" who are often both schizophrenic and megalomaniacal but who can be of service to a society suffering collectively from the same disorders.

MacGaffey is a subtle sociologist, but he is less reliable when he advances out of his research area. He visited Zaire in 1964–66, 1970, and 1979–80. His analysis relates to the tiny prophetic churches, not the principal Kimbanguist *Eglise* whose hierarchy was already well established before MacGaffey first arrived. His brief description of the *Eglise* is well worth reading but does not add greatly to what we already know. Clearly, the church's sustained resolve to be essentially Western and Protestant falls outside the author's sympathy; according the MacGaffey, the *Eglise* was "ideologically coopted by the colonial regime" (p. 209).

As a sociologist of postcolonial Congo religion, MacGaffey is unrivaled; as a historian of Congo religion before 1960, he is not equally to be relied on. "This book began historically, with the Protestant missionary effort of the 1870's" (p. 245), but his account is notably ahistorical. He is far too dismissive of the long history of Christianity in precolonial Congo, and his references are mostly brief, at times confused, and generally overdogmatic and underdocumented. There are several good recent histor-

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ical studies of Congo religion that MacGaffey did not apparently use. He has, however, helped clarify the diversity within current Congo prophetism, Kimbanguist and non-Kimbanguist, ranging from the well-ordered bureaucracy of Diangienda, from which healing has almost disappeared, to the strange imaginings of Mpadi rejoicing in his "khaki trousers of salvation" with all its buttons on. What is no clearer is quite where Kimbangu himself stood within the wide prophetic spectrum.

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ANDRÉ ODENDAAL. *Black Protest Politics in South Africa to 1912*. Reprint. Totowa, N.J.: Barnes and Noble. 1984. Pp. xv, 391. $28.50.

At a time when militant mass-based African nationalism is again resurgent in South Africa, the respectful and elite-oriented activities of the African groups described in this volume seem more than ever from a distant and different era. Yet the evidence and analysis presented in this meticulously researched book convincingly argue that a countrywide basis for African nationalism was firmly laid by these very groups before the establishment of union in 1910.

André Odendaal's account starts in the late nineteenth century with the catalytical role of electoral politics in the Cape Colony, the significance of the new vernacular press, and the impact of Ethiopianism as a prelude to the reactive politics of the post–Boer War period that provide the central focus for the book. Odendaal delineates the distinctive features of the African politics of each of the four colonies and the personal and strategic rivalries between leaders, particularly the crucial cleavage between John Tengo Jabavu, who ultimately placed faith in sympathetic white politicians, and the larger numbers of his contemporaries from all the four colonies who emphasized the imperative of African political organization.

The leaders, drawn from the small minority educated in mission schools, shared a common Christian and liberal ethos, yet Odendaal argues plausibly that they simultaneously cultivated links with traditional leaders and received support from them and their followers, even in neighboring British territories. The carefully argued moderate representations of African leaders to white sympathizers and politicians (in South Africa and Britain) and to colonial and imperial authorities are examined thoroughly, as are efforts at building multiracial coalitions (including coloreds and Indians). Although ultimately unsuccessful in extending the limited nonracial features of Cape Colony politics into the Act of Union, Odendaal shows that African leaders did succeed in drawing together support from all the four provinces into the South African Native Convention, which provided the platform from which the African National Congress was launched in 1912.

The authority of this slightly revised doctoral dissertation by a young South African historian rests above all on the exhaustive research that he conducted in all of South Africa's official archives and in the contemporary press, including substantial use of African vernacular journals. Odendaal has gone beyond the sources that I could have wished to consult in preparing the first part of my *Protest and Hope, 1882–1934*, the opening volume of *From Protest to Challenge: A Documentary History of African Politics in South Africa, 1882–1964*. The roots of twentieth-century black politics in South Africa have been fully traced by Odendaal; no one interested in the antecedents of the continent's earliest African nationalism can ignore this book.

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BRIAN WILLAN. *Sol Plaatje: South African Nationalist, 1876–1932*. (Perspectives on Southern Africa, number 36.) Berkeley and Los Angeles: University of California Press. 1984. Pp. xi, 436.

Solomon Tshekisho Plaatje was a remarkable South African. Consider the scope of his achievements: interpreter to the British civil commissioner during the siege of Mafeking (1899–1900); a founder and the first general secretary of the forerunner of the African National Congress (1912); author of *Native Life in South Africa* (1916), a devastating revelation of the brutal effects of the Natives' Land Act, the first major piece of segregationist legislation passed by the Parliament of the Union of South Africa, and author of *Mhudi*, the first novel written in English by a black South African (1930); and translator into Setswana of Shakespeare's *Comedy of Errors* (1930) and *Julius Caesar* (published posthumously in 1937). Plaatje traveled extensively in North America (1920–22), meeting numerous black American leaders including W. E. B. Du Bois, with whom he struck up a warm friendship.

Plaatje's was a life of perpetual poverty and successive disappointments that presaged the tragedy that has encompassed South Africa since his death. A product of a rural environment and a missionary education, the young Plaatje accepted the promise inherent in the colonial constitutional and legal system, which was theoretically color-blind. As he saw it, his people were destined to become literate and "civilized," and as they did so they would be the beneficiaries of an enlightened imperialism and progessively incorporated into the colonial citizen body. This liberal faith carried him through his formative years and into his mid-thirties. Thereaf-

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ter, all was downhill, as the liberal promise evaporated. Moreover, failing to establish rapport with the burgeoning African population of the industrial cities, he lost his standing among black political activists as the momentum shifted to the towns. By the time of his death, he was a forgotten person.

Brian Willan has identified an outstanding representative of the class of black South Africans who were imbued with Victorian imperial ideals. Searching out and vigorously pursuing numerous clues, he has uncovered a vast amount of relevant evidence. And he has told his story quite eloquently. In doing so, he has maintained a fine balance between probing deeply into Plaatje's life and describing the opportunities and the constraints of the society in which he operated. The result is a brilliant vindication of biography as a significant form of historical writing. It raises fundamental questions about the entire historical process in South Africa since whites conquered the black populations in the nineteenth century. How is one to assess Plaatje's reliance on white allies and his commitment to African "improvement" and progressive incorporation in a civilized society? Was his a realistic program that was destroyed by malicious and shortsighted people? Or was it a pipe dream, a false consciousness instilled by missionaries, lawyers, and politicians? Willan's masterly work provides a basis for mature reflection on this conundrum.

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ASIA AND THE EAST

LINDA GROVE and CHRISTIAN DANIELS, editors. *State and Society in China: Japanese Perspectives on Ming-Qing Social and Economic History*. Tokyo: University of Tokyo Press. 1984. Pp. x, 507. $55.00.

At last we have a book that can be used to introduce beginning graduate students to the immense body of Japanese scholarship on China's social and economic history. Historians trained in the U.S. before the 1960s seldom had a command of Japanese in addition to Chinese, but, since the 1970s, good graduate training has generally come to include a solid grounding in Japanese and Japanese scholarship. Credit goes to John K. Fairbank and others who prepared the two bibliographic guides to call attention systematically to Japanese research on modern Chinese history. Credit goes also to the maturation of the field and to the increasing influence of the social sciences. Still, until the appearance of this book, most students could not read the works themselves until they had acquired sufficient competence in Japanese, often only after finishing their candidacy examinations and going to Japan.

All but one of the pieces selected by the editors come from the so-called Toyko school, but one should not therefore expect doctrinaire Marxist analyses. The lead article by Nishijima Sadao on the early cotton industry, for example, assumes the relative autonomy of the state from socioeconomic structure. We see state taxation altering society and economy in a fundamental way. Tanaka Masatoshi's piece (I prefer the one on popular collective action to the excessively theoretical one on rural handicrafts) is rather more doctrinaire, but it is a good introduction to the kind of class analysis that occupies a prominent place in Japanese scholarship. Also, it is sufficiently empirical to serve well as an introduction to the subject of popular rebellions in the late Ming. Moreover, the student can find a counterbalancing argument in Kobayashi Kazumi's essay, which emphasizes ideology more than class relations. My candidate for the fourth essay to assign is the one by Tsurumi Naohiro, an excellent introduction to the *lijia* system of the Ming and, again, written from a standpoint in which the state stands as semiautonomous actor, not easily collapsible into "the ruling class." All these pieces are well translated and edited.

These essays can also lead into the immense body of Chinese scholarship on "incipient capitalism." All serious students of Chinese social and economic history have to learn to sift out the useful information and insights from the formulaic framework of that body of scholarship, just as of some Japanese scholarship. I would follow up Nishijima's excellent essay, for example, with the recent article by Cong Hanxiang (*Zhongguo shi yanjiu* 1 [1981]: 61–78), which supersedes Nishijima in empirical research, though not in interpretive analysis. I would also direct students to read the recent writings of Li Wenzhi and Liu Yongcheng, which draw on the Qing Board of Punishment archives not available to Nishijima. A week or two of such readings in Japanese and Chinese scholarship can round out the existing American works on the social and economic history of late imperial China.

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IROKAWA DAIKICHI. *The Culture of the Meiji Period*. Edited by MARIUS B. JANSEN. (Princeton Library of Asian Translations.) Princeton: Princeton University Press. 1985. Pp. xvi, 320.

During the 1960s the rise of the "emperor system" was the key issue most Japanese historians of modern Japan felt compelled to address. Defeat in 1945 had revealed that system to be "a hideous miasma, enveloping and subsuming the popular mind" (p.

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13). To exorcise its residual influence postwar historians tried to expose it as an artificial structure contrived by the Meiji regime to shore up its fragile authority.

In this series of loosely connected essays, based on his own original research and on secondary literature, Irokawa asks what Japan was like in early Meiji before the emperor system was put in place. He finds it to be a period of intense experimentation when the urge for modernization and democratization was not confined to the Westernized political and intellectual elite in the new capital at Tokyo. It could be found at work among farmers in remote mountain villages on the edge of the Kantō plain and in the lives of men like Chiba Takusaburō, a political "wandering pilgrim" who ended his life as an obscure schoolmaster.

This book won a wide popular audience in Japan because of the passionate sympathy for the "ordinary people" usually overlooked by academic historians. Irokawa's basic argument is that "ordinary people" had their own ideas about what the new order in Japan should be. These ideas were rooted in a tradition of communal self-government and in a "conventional" or public morality. When ordinary villagers constrasted the harsh capitalist and bureaucratic rationality of the post-Restoration regime with the loose paternalistic habits of the old ruling class, they often rose in angry resistance and rebellion against the new state.

To Irokawa's obvious regret, the halting steps taken toward a less bureaucratic, less statist, and more democratic modernization were ended by the collapse of the *jiyūminken* movement. By the time of the Russo-Japanese War the potential for a popular transformation of Japan had been crushed. The "conventional morality" that once had supported local cooperation, mutual help, and a spirit of autonomy had hardened into a "social, exterior norm" that dictated conformity and passive submission to state authority and goals.

If such a potential for a popular alternative to the emperor system existed, then why did it lose out? In his final essay Irokawa suggests that the "family state" ideology of the emperor system was able to take hold for a variety of reasons: the assiduous propagation of the myth of direct imperial rule; the resonance of official ideology with the values reflected in the family structure; folk morality and the practice of ancestor worship; the use of the education system to indoctrinate the masses; and, finally, the patriotic sentiments roused by the Russo-Japanese War. Irokawa does not, however, make clear how much weight should be given to each of these factors in explaining the establishment of the emperor system.

Nor does he fully explain how values implicit in the popular tradition, which he argues provided a base for resistance to the new Meiji state, could be converted so easily into statist ideology. This is because his main agenda is to challenge the view of Maruyama Masao and some of his followers that Japan could be liberated from the remnants of the emperor system only if it rejected the village and its social values. Early in the book Irokawa refers to this view as the "despair and self-righteousness of empty modernism" (p. 14). As a result, he insists that popular morality and the village community (*kyōdōtai*) did not necessarily provide the foundation on which the emperor system rested. Irokawa also gives short shrift to banal Marxist interpretations of history that reduce its dynamic to economic interests and class struggle.

In sum, Irokawa struggles to find an alternative vision to the two main schools of historical writings in the 1960s—the Marxist and the modernist. He does not quite succeed. Irokawa admits that the book lacks the intellectual rigor, coherence, and consistency to be compellingly persuasive (p. viii). It is also flawed by a romantic nostalgia for a good old pre-Meiji Japan not yet blighted by capitalism or an oppressive bureaucratic state. Ironically, the book is often redolent of a neotraditionalist critique of Meiji modernization common in the 1930s when authentic "Japanese spirit" was contrasted with mechanical Westernization imposed by the Meiji leaders.

Nevertheless, one cannot finish this complex, inconsistent and often rambling collection of essays without coming to a new and deeper understanding of the Meiji period. It not only throws light on the period's darker corners but also raises a new set of questions about its meaning.

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NOBORU KARASHIMA. *South Indian History and Society: Studies from Inscriptions, A.D. 850–1800*. New York: Oxford University Press. 1984. Pp. xxxvi, 217. $29.95.

Noboru Karashima's work combines the best of conventional historical methods with, for early India, the unconventional use of statistical analysis. He is a competent epigrapher, as the best historians of South India have been, and he has also prepared frequency analyses of terms and titles found in inscriptions of the Chola period (ca. ninth to thirteenth centuries). Alas, however, Karashima has seldom sought to examine and to justify the central premises of his historical interpretations, such as the purported significance of private landed property and land sales during later Chola times. Therefore, much of his argument on rural social and economic history of that time will appear deceptively simple to the nonspecialist reader, who might assume that law

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and the institutions in which private property is embedded were extant at that time. And those who know the age will have to treat his work either as another unconvincing claim that medieval India was precociously modern or as unproven assertions and historically unconditioned reasoning about inscriptional terms and structures inferred from them.

Of the thirteen essays in this volume, several first appeared in monographs written by Japanese scholars working in South India and published by the Institute for the Study of Languages and Cultures of Asia and Africa, Tokyo, with which Karashima is affiliated. Other essays are newly translated from the Japanese, and a few are collaborative studies with Indian scholars.

The framing of these several valuable essays is something of a problem, for Karashima has chosen to pit his findings against the general formulations of Kathleen Gough and the present writer. According to Gough, the Chola state exemplified Marx's "archaic state" under an Asiatic mode of production managed by a theocracy and based on hydraulic agriculture and slave labor. Karashima rejects this interpretation because it fails to account for private landholding, to deal with the extensive Chola territory in which there was little irrigation, and to examine coherently the purported slavery of the age. My view of the Chola kingdom as a political order unified by a nonbureaucratic, ritual kingship whose parts were communally organized under dominant peasant groups is rejected by Karashima as "too speculative." Karashima holds that it is the "clarification of more specific problems and not the presentation of any theoretical framework" that is wanted (p. xxv). As he concedes "the prevalance of communal landholding" in most villages (p. 14), I am struck by the "speculative" and unexamined "theoretical" propensity of Karashima to take most land transfers as evidence of private landholding and sales. It is also striking that he takes the relatively insignificant number of these, according to his own statistics, to constitute a major feature of the Chola rural economy and also has, therefore, to posit what he cannot demonstrate, that is, political and legal institutions that protected this purportedly private property.

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R. SUNTHARALINGHAM. *Indian Nationalism: An Historical Analysis*. New Delhi: Vikas. 1983. Pp. viii, 471.

A short review of a large and complex work such as this can provide little more than a bare appraisal of the book's contents and relative value. R. Suntharalingham, a distinguished Malaysian scholar, derived his data primarily from an impressive array of publications issued during the past fifty years by specialists in South Asian history. Thanks to his mastery of the English language, his skill in drawing interpretations, and his consistent objectivity, the book is eminently readable.

The study is organized chronologically in topics, each of which includes three to five subdivisions. At the outset Suntharalingham defines nationalism as a distinctly modern concept associated with the French Revolution, thereby disposing of Annie Besant's mythical Aryan attribution, a view shared by Indian exponents of the Romantic movement like Radhakhand Mookerjee. In part, Suntharalingham also rejects M. N. Roy's strict Marxist interpretation of Indian nationalism, whereas he accepts this reviewer's theory of the English educated class as a prime agent in embracing and propagating early Indian nationalist doctrine—a view akin to that of Anil Seal with regard to group motivation.

Concerning the origins of nationalist ideology in India, Suntharalingham notes that the British introduced the ideals and standards of the modern European state, such as the rule of law, curbs on arbitrary government, belief in representative institutions, a new conception of the state's role in society, and the idea of nations separate but equal, possessing the right of self-determination. He cites the changes wrought in Indian society by British economic and educational policy that contributed to the emergence of Indian nationalist ideology. In the economy these alterations caused the breakdown of the self-sufficient Indian village and other changes in production that laid the basis for new classes with which to form an Indian capitalist society. British rule also intensified trading activity in the coastal regions, especially Bombay, Bengal, and Madras, where adaptive Indian groups gained wealth and social mobility, while radical changes in landownership converted land into a flexible market commodity. Along with the creation of a small, powerful landowning elite, the nineteenth century witnessed a slow, unsteady transition toward the establishment of modern industrialism and the emergence of two new classes: the bourgeoisie and the proletariat. The introduction of Western education by the British raj created an Indian intelligentsia with a bourgeois sense of identity and aspirations for government employment that collided with the alien bureaucracy, causing tensions that aligned the educated Indians in nationalist associations to promote their class interests.

Subsequent chapters analyze the rise of the Indian National Congress, the Muslim response to incipient Indian nationalism, Gandhi and the "Non-Cooperation" program that transformed the Congress into a mass movement, ideological conflicts within Congress on the rise of a left wing, the bloody

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clashes of Hindus and Muslims in Gandhi's civil disobedience campaigns, and the culmination of the quarrel between Muslims, Hindus, and the British raj in the grant of independence and partition of the subcontinent between India and Pakistan in 1947, at a cost of two million lives. It is safe to say that Suntharalingham's splendid book will be haled as the definitive study of Indian nationalism for years to come.

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JOHN HILVERT. *Blue Pencil Warriors: Censorship and Propaganda in World War II*. St. Lucia: University of Queensland Press. 1984. Pp. vi, 258. $27.95.

The controversy surrounding the press coverage of the invasion of Grenada is a recent example of a long-standing debate: how far should a liberal democracy go in the exercise of censorship and propaganda during a war or military action? This book by John Hilvert illustrates how Australia tackled the question during World War II.

It is essentially an administrative history, a study of the Department of Information, which was created on September 4, 1939, the day after Australia entered the war, and abolished in March 1950. Hilvert charts the arguments surrounding the department's activities in its twin activities of suppression and expression. The inevitable conflicts between the censors and the censored are well documented.

Hilvert concentrates on the personal and political relationships between ministers and senior officials. The most notable of these were Sir Keith Murdoch (father of Rupert), the press baron who became the department's first head; Arthur A. Calwell, who advocated the department's abolition before becoming its minister; Edmund Bonney, the tough-minded chief censor; and W. Macmahon Ball, the liberal academic in charge of shortwave broadcasts. Hilvert devotes a separate chapter to the events of April 1944, when the major Australian newspapers were suppressed.

Hilvert succeeds in adding to what has previously been written on the subject and in challenging some previously accepted interpretations. The book is written in a rather flat and humorless style; academically, it is also rather unadventurous. The subject is not set in a theoretical context, nor are there comparisons with the experience of other countries during the same or other wars. For a start Hilvert could have included a comparison with Australian censorship during the 1914–18 war, covered in an excellent, but unpublished, dissertation by Kevin Fewster. Nevertheless, Hilvert, who is now a public servant employed to implement the Freedom of Information Act, has written a useful addition to scholarly literature.

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UNITED STATES

PETER GARDELLA. *Innocent Ecstasy: How Christianity Gave America an Ethic of Sexual Pleasure*. New York: Oxford University Press. 1985. Pp. viii, 202. $17.95.

Christian theologians have long debated the role of sex in the divine scheme of things. Some have said that God, having given men and women the possibility of sexual pleasure, meant them to enjoy it without guilt. Others have identified sex with original sin and argued that sexual relations must always be tainted by guilt. Peter Gardella traces the changing attitudes of Catholic and Protestant theologians toward sex from medieval times to the present. Followers of Saint Augustine, stressing the guilt implicit in sex, tried to limit it to procreative purposes and "led some to conclude that any consent of the soul to the pleasure of sex was sinful" (p. 11). Pope Gregory the Great said that even "married people should not receive communion after intercourse unless they had first done penance" (p. 11). Thomas Aquinas, however, argued on the basis of natural law that intercourse in marriage was a legitimate act and, hence, "the pleasure produced by that act did not entail even venial sin" (p. 12). Martin Luther "enjoyed sex and he taught that men and women had a fundamental right to sexual enjoyment," although men and women must beware of "bestial desire and lust" (p. 5).

Since Luther, Gardella maintains, the doctrine of original sin has altered religious attitudes toward sex in three distinct phases. In the eighteenth century, "Catholic moralists made orgasm obligatory" but hemmed it in by emphasis on procreation within marriage (p. 150). "Then Victorian physicians and theologians diagnosed original sin as sexual disorder" (p. 150) and insisted that science could overcome that disorder by carefully regulating sexual conduct (some doctors claimed that married couples should limit sexual relations to once a month or perhaps once every three years if they wanted to be healthy in mind and body). In the twentieth century, psychologists and birth control advocates "taught that sin could be overcome by sex" if it were carried out with mutual concern for orgasm (p. 150). Today the perfect marriage for most Americans rests on orgasmic sexual performance. Gardella concludes that this has placed a terrible burden on sexual

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partners, each seeking to prove their ability to produce and experience "innocent ecstasy" every night. Asserting that this has gone too far, Gardella concludes that "the pressure to practice sex well would decrease if Christianity recovered the doctrine of orignal sin" (p. 159).

This book is too sophisticated, well written, and carefully analyzed to bear crude summary. Gardella is a serious cultural historian, and this study is based on extensive and solid research. Gardella is really concerned with the secularization of religious thought in America. Catholics as well as Protestants, liberals as well as Fundamentalists, have yielded to the power of popular culture as Americans sought to perfect themselves and the world through free will and scientific control. The book relates the changing conceptions of sin and sex to America's great awakenings, to the rise of Mariology, to Methodist camp meetings, to the Know-Nothing movement, to Pentecostal and Holiness revivalism, to Christian Science as well as to the feminist movement, gynecology, dietetics, evolution, psychoanalysis, and contraception. "By associating birth control with happy marriage and healthy motherhood," Margaret Sanger "addressed the yearning for freedom from sin," which has made America such a perfectionist culture with such an overwhelming urge to dominate and exploit nature (p. 133). Although some of Gardella's efforts to link Christianity to sexual pleasure seem a bit forced, the book is highly imaginative, original, and provocative. He has successfully placed sex in its historical context and provided a new perspective on the relationship between religion and culture in America.

WILLIAM G. MCLOUGHLIN
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BRUCE KUKLICK. *Churchmen and Philosophers: From Jonathan Edwards to John Dewey*. New Haven: Yale University Press. 1985. Pp. xx, 311.

Bruce Kuklick's new book surveys the history of American Congregational philosophy from Jonathan Edwards to John Dewey. It is the first comprehensive, serious treatment of the subject since Frank Hugh Foster's *Genetic History of New England Theology* (1907). As the subtitle indicates, two great figures dominate the book—Edwards at the beginning and Dewey at the end. Kuklick's principal contribution lies in his treatment of these two figures. The discussion of Edwardsean philosophy proceeds mainly along the lines of Norman Fiering and Wallace Anderson. Kuklick provides the best assessment we have of Edwards's influence in creating the Congregational intellectual tradition and an excellent orientation to Dewey's relationship to it. Although locating Dewey in the context of American Congregational theology may well be considered controversial, there is ample justification for doing so.

In *The Rise of American Philosophy* (1977), Kuklick dealt with early twentieth-century Harvard; in the present work the focus shifts to nineteenth-century Yale, with Andover, Princeton, Harvard, and other institutions on the periphery. Yale, Kuklick maintains, was the philosophical capital of the United States as long as Calvinist theology remained the dominant philosophical school of thought. Yet the middle portions of this book are not quite so strong as the beginning and the end. D. H. Meyer's *The Instructed Conscience* (1972) discusses many issues in the technical philosophy of mid-century American academia that Kuklick passes over. And, not surprisingly, Kuklick writes about the Unitarians as the Calvinists perceived them, without appreciating the internal coherence of the Unitarian position.

In a book about nineteenth-century American philosophy, to take no notice of the changing parameters of the subject is peculiar. Moral philosophy, as defined at the beginning of the century, included all of what we today consider the social sciences. Noting this would have helped Kuklick show Dewey's relationship to the Congregational tradition. Dewey's "great achievement," Kuklick observes, "was incorporating what were recognized at the time as religious values into a scientific conception of man and nature" (p. xx). By synthesizing religious philosophy with social science, Dewey was re-creating something like the moral philosophy of old-time American Protestantism.

Whether Kuklick is arguing a thesis or presenting a general survey of a topic is not always clear. But his assessments of individuals and movements are usually sound (I particularly liked his summation of the New Divinity), and he has provided a valuable, up-to-date treatment of what is "the most sustained intellectual tradition the United States has produced" (p. 222).

DANIEL WALKER HOWE
University of California, Los Angeles

JAMES TURNER. *Without God, Without Creed: The Origins of Unbelief in America*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xviii, 316. $26.50.

James Turner's study aims to trace the emergence of agnosticism as a "fully available option in American culture" (p. 262), a process he tracks through the two centuries between 1690 and 1890. Downright nonbelievers, he argues, emerged in European culture only in the eighteenth century, and then

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only as a minority within the ranks of advanced thinkers who inclined toward "atheistical speculation." Most notable American intellectuals clung to a "legitimate if thinblooded form of theism" (p. 46). The existence of a God was necessary to the Enlightenment belief in the order of a universe whose human inhabitants derived their moral laws from the laws of nature. Nonetheless, numerous Christian apologists decried the growth of "atheism." In this clash church leaders were forced to "rethink the nature of God and the justifications for belief in Him," a process "more far-reaching than, though continuous with, that begun between the Reformation and about 1690" (p. 49).

In casting about for a formulation that would be intellectually respectable as well as persuasive to the popular mind, churchmen fastened onto science as the explanatory model by which belief in the existence and attributes of a benevolent God could be made rationally demonstrable. Their idea of what science was and what it could fully explain would seem cheerfully naive (or tragically clouded) to a generation of intellectuals who view the workings of science in Thomas Kuhn's terms. To eighteenth-century churchmen and their successors, science was an all-sufficient and glamorous heuristic device. Any idea that could be made plausible through analogy to science would command, they trusted, both the intellectual assent and emotional trust of intelligent believers. This is the central irony of Turner's book: by pursuing the goal of rational belief to the almost total exclusion of other modes of religious thought, the rationalizers of Christianity paved the way for full-blown unbelief.

As Turner ably demonstrates, every aspect of Christian doctrine came to be tried by the touchstone of science. The "divided God of the Enlightenment," a conception that set belief in a personal deity against God the designer, became the reigning divinity in America. In Turner's intricate analysis the aggressive exponents of "heart religion" were also the arch-rationalizers. Against this phalanx of rational defenders of God's existence and purposes were ranged such intuitionist luminaries as Ralph Waldo Emerson and Horace Bushnell, who rightly grasped that scientific method as applied to religion limited one's knowledge of God. Their voices, however, could hardly compete with those who claimed that certain knowledge could be gained by emulating science.

Public fascination with the triumphs of technology (easily confused with science) and the attractions of Darwinian evolution guaranteed that the language of science would dominate the language of religion in the 1860s. Opponents of this trend were handicapped, Turner argues, by the paucity of competing ideas. Meanwhile, prophets of the cult of the heart (like Henry Ward Beecher) embraced evolutionary science as their own. The foundations of American intuitionism were in any case too slender to prop up a viable alternative to scientistic religion.

In this atmosphere many intellectuals came to believe that what could not be known via scientific method was surely not worth knowing. Thinking in this fashion corroded religion to the point where intellectuals could dispense with God altogether. The number of avowed nonbelievers was small, but it was not only possible but also intellectually respectable to be agnostic. Unbelief, once "a rare eccentricity," could now "stake out a sizable permanent niche in American culture" from about 1880 onward (p. 171).

Turner's sympathetic and sensitive handling of these themes gives us further insight into the structures of belief of a society that, as he says, wanted to be comfortable in a universe tailored to human measurements.

MARIE CASKEY MORGAN
Eaton, New Hampshire

DAVID ROBINSON. *The Unitarians and the Universalists.* (Denominations in America, number 1.) Westport, Conn.: Greenwood. Pp. xiii, 368. $35.00.

David Robinson has written the first book in the Greenwood Press series on denominations in America. Henry Warner Bowden, author of *The 1977 Dictionary of American Religious Biography*, states in his foreword that "[this] series follows a distinguished precedent. . . , the prototype [of which] appeared almost a century ago" (p. ix). He refers to the American Church History series (1893–97), the result of collaboration between twenty-four scholars. But improvements in historical method and a better understanding of the place of religious development in the broad context of American studies have enabled David Robinson to produce a major contribution to the intellectual history of the United States.

Following in part the guidelines of Vernon Louis Parrington and Perry Miller, Robinson traces the origins of both Unitarianism and Universalism from late colonial times through nineteenth-century secularism, the trauma of the Civil War, and the Free Religious movement of the postbellum period to twentieth-century humanism. Robinson clearly analyzes parallels and differences in the developments of the two denominations. The reader gets the impression the differences were more important than the similarities: Unitarians were upper-class, enlightened, New England rationalists, whereas Universalists were less well educated, following the heart over the head in matters of religion. Why did the two groups merge to form a single denomina-

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tion in 1961? Robinson's historical presentation makes clear that both groups had one essential attitude in common: their dedication to a creedless, nonauthoritarian religion. They also had, and still have, one problem in common: how to reconcile a free church with the practical matter of necessary organization—a tension that has troubled liberal churches throughout our history. Robinson also brings out another conflict within the Unitarian-Universalist tradition: the conflict between intellectual and rational approaches to religion. This led to different attitudes toward social issues and social action, for Unitarians lean less toward social action than Universalists (although this might be debated when individuals are studied).

Part 1 of Robinson's study is analytical history. Chapter 1 is a well-written survey of the roots, similarities, and contrasts found in Unitarianism and Universalism. Succeeding chapters deal separately with the rise and growth of each denomination. Those familiar with the sources and bibliographical studies relating to both denominations will find nothing new in any particular chapter. Robinson presents his book, however, not as a piece of original research but rather as a synthesis of well-known primary sources and secondary works. This is no adverse criticism since Robinson's well-researched and lucidly written book accomplishes what the series intends—denominational history. He deserves to be commended highly for his scholarly efforts. One minor gap might be pointed out by this reviewer. If the author had studied the annual *Proceedings of the Free Religions Association*, he would have established a stronger and earlier case for the growing affinity between Unitarians and Universalists. After all, most of the people associated with that organization were Unitarians, and many major speakers at annual meetings were Universalists.

Part 2 of the book is a 32–page biographical dictionary of leading figures in Unitarian-Universalist history. All scholars in American studies and intellectual history will find this an invaluable reference work.

J. WADE CARUTHERS
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MARY CHARLES BRYCE. *Pride of Place: The Role of the Bishops in the Development of Catechesis in the United States*. Washington, D.C.: Catholic University of America Press. 1984. Pp. xii, 227.

This book's subtitle, which might be amended to read "of the Roman Catholic Bishops," is otherwise precise. This is neither a history of religious education in the Roman Catholic Church in the United States nor a history of catechetical manuals used for that religious education. The focus is on the bishops, first individually and then collectively. The title is taken from the Second Vatican Council's decree on the pastoral office of bishops in the church (October 28, 1965), which notes that, among various methods available for proclaiming Christian doctrine, preaching and catechetical instruction hold "pride of place."

The first third of the book describes in episodic fashion a dozen or more efforts to introduce various catechisms of European provenance into the United States. The discussion begins with the 1785 manual that was erroneously named, as the author notes, for John Carroll, the recently appointed ecclesiastical superior of the Catholic church in the new republic; it was actually a rendition by Philadelphia pastor Robert Molyneux of older English manuals. Catechisms that failed to gain episcopal approval are also mentioned, including the one written by Caesarius Reuter in the late 1790s for Pennsylvania German congregations. But Mary Charles Bryce makes no reference to the controversy in 1791 between Carroll and Francis Rogatus Fromm, who attempted to replace the German version of the Counter Reformation catechism of Robert Bellarmine with one of his own making.

For the general historical background, Bryce relies heavily on the ultramontane historiography of Peter Guilday, who wrote in the 1920s, with some additional material from authors of the 1950s. The result is a somewhat dated reading that neglects, for example, the more benign view of controversies over the role of lay parish trustees that can be found in recent studies by Patrick Carey. Bryce's approach also makes it difficult for her to integrate the emphasis common in the Anglo-American Catholic community of the early nineteenth century on the notion of national churches that insisted on their national character while proclaiming themselves "in communion with the see of Rome." Surprising is the omission of any reference to the widely used "Keenan's Catechism," which before the First Vatican Council taught Catholics to reject as "a Protestant invention" the charge that they accepted a doctrine of papal infallibility.

At the first national meeting in 1829 at Baltimore, the bishops in formal council began the quest for a common catechism, which resulted in the first "Baltimore Catechism" in 1885. Bryce chronicles well the intermittent discussions and aborted projects that led to the graded set of question-and-answer manuals that served American Catholics until the post–Vatican II period, when emphasis shifted from mandated, specific textbooks to biblical study and general directories. Catechetical renewal became international, inspired by the General Catechetical Directory produced in Rome in 1971. Bryce, active in the field during this last phase, is at her best in describing this development, and her

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study, while still stressing episcopal participation, becomes a more general history of religious education as she carries the story down to the 1977 international synod of bishops on the theme "Catechesis for Our Time" and subsequent publication of the American bishops' *National Catechetical Directory* (1979).

JAMES HENNESEY
Boston College

JACK D. MARIETTA. *The Reformation of American Quakerism, 1748–1783*. Philadelphia: University of Pennsylvania Press. 1984. Pp. xvii, 356. $27.50.

The history of eighteenth-century Pennsylvania, like that of seventeenth-century Massachusetts, requires an integration of religious, political, and social history. The members of the Society of Friends, a minority in the colony even before the 1720s, managed to keep political power until the eve of the revolution. In this exhaustively researched, clearly written, and provocative book Jack D. Marietta shows the importance of a group of reformers in changing many Friends' preoccupation with wealth and political power to concentration on inward piety and social transformation. He demonstrates that the Quaker renaissance between 1750 and 1780 reversed the sect-church sequence described by Richard Niebuhr; in Pennsylvania a churchly organization voluntarily abdicated political power, becoming more strict, more cut off from the world, and more sectarian. Quakers also did not follow the pattern of religious revitalization described by William McLoughlin, for the meeting repudiated the techniques of evangelical awakenings, willingly accepted decreasing numbers of members as a price of purity, and heightened the imbalances between the general culture and the approved Quaker pattern of life. The hypothesis that Americans embraced social reform as an attempt to regain status also proves inadequate. Quakers advocated strict pacifism, Indian rights, and abolition partly because such unpopular crusades brought a decline in status.

The book has two distinct but interrelated themes. The first half is a description of how a small number of Friends, disturbed by the compromises that prosperity and politics entailed, managed to purge the meeting of those unwilling to fulfill what the reformers defined as traditional Quaker norms of behavior. Through painstaking work Marietta has compiled the names and offenses of all Quakers brought before the meetings in Pennsylvania from the 1680s until 1780 so that he can chart changes in the timing and nature of discipline. He demonstrates that, after a gradual but steady increase in the number of offenses from 1700 to 1750, disciplinary proceedings increased dramatically after 1755. Pennsylvania Quakers disowned over 20 percent of adult Friends between 1760 and the revolution, primarily for marriage with non-Quakers and war-related issues. In their quest for purity the reformers succeeded in adding testimonies against holding slaves, paying war taxes, and serving in government.

A second section recounts the history of Quakers in politics and the relationship of factions within the meeting and the assembly to events from the 1740s until the revolution. Marietta shows how the rhetoric of conservative Quaker merchants, members of Quaker Joseph Galloway's faction, and the epistles of the Meeting for Sufferings convinced patriots that Friends were Tories. Those who most actively sought to persecute Friends during the war with Great Britain were disowned members. Quaker leaders took advantage of the popular hostility to Friends to complete the countercultural reformation begun in the 1750s.

The Quaker reformers—John Woolman, Anthony Benezet, the Pembertons, the Churchmans, and a few English Friends—left eloquent writings of their activities. The compromisers, the moderates, and the disowned left far less material, and Marietta shows little sympathy for, or interest in, them. He sees the creative element in Quakerism as coming from the vision of a minority dedicated to abstract and rather rigid ideals of behavior. Unlike Marietta, Jean Soderlund's research on antislavery shows that the close relationship between sectarianism and emancipation did not exist in all areas. Barry Levy's reconstruction of families in Chester County, an area very strict in enforcing discipline, shows a strong correlation between having wealth and staying within the meeting. The poor were more likely to be disowned, a conclusion contrary to what one would expect from Marietta's account. I also am not persuaded that the reformers of the 1750s began or remained a cohesive group until the 1780s, consistently favoring strict discipline, voluntary poverty, withdrawal from government, and abolition. At issue is whether the reformers' ideas and actions or economic, cultural, and social changes plus two major wars were the critical factors in transforming Pennsylvania Quakerism. Marietta has provided firm evidence that a small elite played a role in redefining the attitudes of Friends toward each other and outsiders. He has revised previous interpretations by Isaac Sharpless, Frederick Tolles, Sydney James, and David Brion Davis and given our best account of the Quaker role in the events leading to the ending of William Penn's "Holy Experiment" in 1776.

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A. OWEN ALDRIDGE. *Thomas Paine's American Ideology.* Newark: University of Delaware Press. 1984. Pp. 327. $38.50.

This book is a thorough analysis of Thomas Paine's political writings from 1775 to 1787. A. Owen Aldridge painstakingly identifies as many of Paine's pamphlets and newspaper articles as possible (including some important pieces not previously identified as Paine's), summarizes and criticizes their contents, explains their relation to important political philosophers, and compares them to the politics and polemical literature of the time. The overall thesis—that Paine's American writings are generally consistent with his later European works and that he is a relatively "moderate" philosopher whose views both "do not differ fundamentally from Locke's" (p. 136) and are such that he and Rousseau are "kindred spirits" (p. 146)—is generally clear and plausible. Aldridge argues further that, although Paine cannot be regarded as very close to Montesquieu, he was nonetheless not the dogmatic devotee of an all-powerful unicameral legislature that critics have often pictured him to be. Aldridge thus places *Common Sense* in a broad context of European thought that will be useful and interesting to students of political philosophy. He also identifies and summarizes many contemporary polemicists, both those who supported and those who opposed Paine. The result is that we know not only what the well-known tract *Plain Truth* said in refutation of *Common Sense* but also what "Cato," "Rationalis," "The Deceiver Unmasked," John Adams, "True Merits," and some other minor respondents said about Paine's great tract. Altogether, Aldridge makes a substantial contribution to our understanding of Paine's American writings and their place in the polemical literature of the revolution.

Nevertheless, this is not a profound or intellectually exciting book. Aldridge spends far too much time on dubious efforts to establish influences on and authorships of anonymous tracts by comparing texts. Attempts to compare Paine and Bernard Mandeville (pp. 64–65) seem far-fetched, for example, and, although Aldridge is probably correct in declaring Paine the author of the pamphlet "Four Letters on Interesting Subjects," published in Philadelphia in the critical weeks before the Declaration of Independence, the "parallels" Aldridge notes between "Four Letters" and Paine's other writings are not nearly as impressive or conclusive as he thinks they are. Three chapters (forty-four pages in all) expounding on *Common Sense*, though interesting here and there, are on the whole rather tedious and hardly worth the effort. Most unprofitable of all, Aldridge has a way of dithering over details and commenting on the obvious, while missing more subtle and searching points. Thus, he points out that Paine was both a "primitive," in that he saw precivilized ages as times of virtue and goodness, and a "modern," in that he thought the present and the future much more progressive than ancient times. Then Aldridge announces that this is inconsistent, as indeed it is literally, but he misses the key point that both primitivism and faith in progress scorn and condemn existing institutions and hence share a critical view of the status quo. *Thomas Paine's American Ideology* is thus a useful book by a veteran, learned scholar of eighteenth-century literature in general, and of Paine in particular, but it will not rank as a probing, landmark work in our understanding of American revolutionary thought.

RALPH KETCHAM
Syracuse University

FRED ANDERSON. *A People's Army: Massachusetts Soldiers and Society in the Seven Years' War.* Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Virginia. 1984. Pp. xii, 274. $25.00.

Clio is not comfortable dealing with Mars, although the general public eagerly purchases histories of wars. The profession at large realizes, correctly, that the traditional "drum and trumpet" approach rarely employs the full range of the historian's skills. During the past two decades a new generation of scholars dedicated to exploring the broader social, economic, and political contexts of military history has begun to emerge.

Through the Institute of Early American History and Culture, colonialists have taken the lead in applying new insights to military subjects. Starting with John Shy's perceptive "A New Look at Colonial Militia" (*William and Mary Quarterly*, third series [1963]: 175–85), they have seized on the fact that armies generate more documentation than any other area of society and have subjected that data to many different types of analysis. Through the institute's support, Fred Anderson has built on his dissertation and emerged as one of the top scholars of the "new" military history.

This masterpiece, which won the Jamestown Prize, does not focus on campaigns or generals. Instead, its sections and appendixes examine Massachusetts's military service in the Seven Years' War and the war's impact on colonial society and its individual members. Chapters cover the context of service, the physical nature of the soldiers' experiences, and the colonists' understandings of and attitudes toward service and warfare. Statistical analysis and a careful study of the participants' own accounts provide impeccable underpinnings to a pleasing narrative style.

A short review cannot begin to do justice to the

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scope of this inquiry, which ranges from the effect of diet on behavior to imperial policy. Anderson will force us to reexamine many basic assumptions concerning colonial military service. He conclusively proves that in this war Massachusetts soldiers (one in three males of military age served) reflected a representative slice of society, not its dregs. His discussion of the mix of contractual and religious motivations clarifies issues raised, for example, by Charles Royster about the revolution. Alan Rogers's suggestions about the impact of the war on prerevolutionary attitudes on both sides of the Atlantic is amplified by Anderson's detailed research.

Every reviewer finds issues that he wishes received more coverage. Anderson points out that Massachusetts units received virtually no training but does not relate this to basic militia instruction (pp. 75–78). Nor does he tie his finding that officers developed resentment for their British counterparts to the raising of regiments in 1775 (p. 112). His discovery of a high rate of turnover among officers needs to be balanced against preliminary studies of other colonies that find an opposite trend (pp. 51–52). Finally, his study of battle accounts in chapter 5 would have benefited from the army's post-action interview methodology developed by S. L. A. Marshall during World War II. These minor criticisms in no way detract, however, from a superlative piece of scholarship. The volume is "must" reading for every student of military, social, or early American history.

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RONALD HOFFMAN *et al.*, editors. *An Uncivil War: The Southern Backcountry during the American Revolution.* (Perspectives on the American Revolution.) Charlottesville: University Press of Virginia, for the United States Capitol Historical Society. 1985. Pp. xvi, 346. $25.00.

These ten essays were originally presented at a symposium of the United States Historical Society and the Institute of Early American History and Culture in Washington, D.C., on March 18–19, 1982. With the exception of Jack Greene's introductory essay and that of Richard R. Beeman comparing the Virginia and Carolina backcountry, each essay is confined to the backcountry of one colony. The essays show many similarities in the southern backcountry. The area was made up of newer settlers with more diverse backgrounds than the older settlements. All the authors apparently agree with Greene's idea that what most backcountry settlers wanted was individual freedom and economic development.

Virginia had less trouble between Whigs and Tories in its backcountry because, as Greene and Beeman say, the colony extended responsible self-government to the western area as it was settled, in contrast to the Carolinas, neither of which had done this by 1770. Hence, those colonies' governments had less regard for what backcountry settlers wanted.

Rachel N. Klein says that in South Carolina personal leadership, rather than class, had most to do with decisions of individuals to be Whig or Tory. In his discussion of the establishment of postwar order Robert M. Weir says that the Whig leaders reluctantly backed confiscation and banishment legislation to protect their own position and gradually lessened its rigors once war hatreds had cooled.

A. Roger Ekirch and Jeffrey J. Crow go over much of the same ground for North Carolina but with several differences on fact and opinion. Both agree that much of the backcountry was hostile or indifferent to the revolutionary cause at its beginning. Ekirch says that the Whigs tried hard, with some success, to win the allegiance of the backcountry and that what the people wanted was peace, law, and order. Savage partisan fighting developed between Whigs and Tories in 1780–81 to gain the support of backcountry loyalists, who, Crow says, developed out of prewar discontent with colonial administration. His description makes this fighting seem much worse than does Ekirch's account. Crow provides more facts, but Ekirch gives a clearer picture of what happened and what events meant.

In the only essay that treats a single part of a colony versus backcountry, Emory Evans gives a factual account of troubles in southwestern Virginia. This area had been recently settled, mainly by Celts, and was too far from eastern Virginia to be much influenced by its Whig orientation.

Edward Cashin explains the great desire of Georgia backcountry people for more land. He says, in fact, that this was the whole purpose of their fighting—a statement that some would question. Harvey Jackson emphasizes Georgia's political factions, which combined and fought each other for control of the colony, with the power of the backcountry always important and certainly dominant by the end of the war. W. W. Abbot gives an excellent summary of the historiography on revolutionary Georgia and identifies subjects that he hopes future historians will undertake.

These essays make clear that the politics of the revolutionary era in the southern backcountry were complicated and confused—and often dangerous to the inhabitants. This makes one wonder how the troubles could have ended as easily as they did once the fighting stopped. Most of the essays are well done and give enough facts to make clear what happened and enough interpretation to make the

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authors' viewpoints clear. There are few, if any, startling new ideas, but the essays sum up well existing knowledge.

KENNETH COLEMAN
University of Georgia

MARTIN S. PERNICK. *A Calculus of Suffering: Pain, Professionalism, and Anesthesia in Nineteenth-Century America.* New York: Columbia University Press. 1985. Pp. xiii, 421. $35.00.

This book by Martin S. Pernick focuses on the reasons why anesthesia was selectively used by American physicians during the middle decades of the nineteenth century. Although the use of ether and chloroform as painkillers in surgery was discovered in the 1840s and the knowledge of this discovery spread with unprecedented speed, American physicians were self-consciously selective in their use of anesthesia until the 1880s. Pernick attributes this conservatism to an effort by "orthodox" physicians to find a middle course between the "heroic" practices pioneered by Benjamin Rush and the "naturalistic" approach to medicine typified by "sectarian" practitioners such as homeopaths and hydropaths. Pernick also emphasizes the ways in which nineteenth-century attitudes about age, sex, race, class, nationality, and temperament worked against the universal application of anesthesia.

Drawing on information garnered from nineteenth-century medical textbooks, journals, and lectures delivered at medical schools, Pernick determines that most physicians carefully measured the benefits and drawbacks of anesthesia. This "calculus of suffering" resulted in codification of detailed rules governing the usage of anesthesia. Because it was generally believed that women and children were especially susceptible to pain, anesthesia was more likely to be prescribed for them than for men. Indeed, healthy white men were expected to endure much suffering without the benefit of anesthesia. Poor people, Indians, and blacks were believed to be insensitive to pain and, therefore, rarely received anesthesia.

The strength of this book lies in the detailed, though somewhat repetitive, way in which Pernick examines why and how anesthesia was selectively used by mid-nineteenth-century physicians. Yet he also attempts to draw on such a wide range of attitudes and beliefs that he does not develop any of these in a full and systematic way. Moreover, Pernick is not totally successful in coming to terms with what may be the larger implications of his own arguments. For example, he compares what he labels the "passive" approach of natural healers with William Lloyd Garrison's "passive" strategy of moral suasion and nonresistance to end slavery. It is dubious whether the terms "active" and "passive" accurately describe the conflicts within nineteenth-century medicine or the debate within the antislavery movement over the means of effecting the abolition of slavery. Jolting analogies of this type appear intermittently throughout the book.

Pernick becomes so engrossed with exploring the social and ideological implications of his work that he sometimes overlooks or ignores the obvious. For example, he appropriately includes substantial information on the work of William T. G. Morton and his promotion of surgical anesthesia, but he makes only one oblique reference to the ether experiments of Crawford W. Long. He also pays minimal attention to how the growing use of anesthesia affected the status and position of the surgeon. Finally, Pernick takes only a cursory look at the circumstances under which the poor, Indians, blacks, healthy white males, and others came to be provided anesthesia by 1880. This is particularly troublesome because most of the attitudes in the 1840s about sex, race, and class were still prevalent in the latter period.

JUDY BARRETT LITOFF
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RICHARD SLOTKIN. *The Fatal Environment: The Myth of the Frontier in the Age of Industrialization, 1800–1890.* New York: Atheneum. 1985. Pp. xiii, 636. $37.50.

Richard Slotkin's *The Fatal Environment* continues the inquiry begun in his first book, *Regeneration through Violence* (1973), which was awarded the Beveridge Prize. The two volumes (running to three hundred thousand words each) trace on a grand scale the formation and development of a frontier myth as a central component of American popular culture. By "myth" (or "ideology"—Slotkin uses the terms interchangeably) he means a cluster of recurrent images or symbols that purport to express shared experiences and purposes of a particular group. Once created, the myth or ideology imposes itself as a virtual category of perception, a grid that filters out contravening instances and replaces direct observation with a fictitious version of reality.

The earlier book, covering the period 1600–1860, deals with a frontier defined as the ever-moving boundary between agricultural settlement and the wilderness beyond. Its protagonist, an imaginative version of Daniel Boone, reached avowedly fictional embodiment in James Fenimore Cooper's Leatherstocking. The present volume offers the arresting hypothesis that the impact of the Industrial Revolution brought about a basic change in the conception of the savage enemy so that it embraced not only native Indians but also free blacks in the South, militant industrial workers, and the immigrants who were crowding into the ghettos of eastern cities.

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These groups came to be perceived as "the dangerous classes," and the protagonist with whom the mass audience was invited to identify added to his original functions of Indian fighter and pathfinder aristocratic and managerial traits associated with southern plantation owners, together with the managerial talents of tycoons such as Jay Cooke and Cornelius Vanderbilt. Thus, in Slotkin's view, the frontier myth came to bear a reactionary political charge while still seeming to support progress and civilization against perverse vestiges of primitive backwardness.

In limited space it is of course impossible to note even cursorily the many examples that Slotkin adduces. He shows impressive skill in documenting the transformation of the earlier myth by examining vocabulary, imagery, and even the placement of stories on the front pages of the mass-circulation newspapers that emerged in major cities during the decades following the Civil War.

Slotkin's prime example—and it is a brilliant choice—is the reporting of George A. Custer's "Last Stand," his defeat and death along with the entire detachment of the Seventh Cavalry under his command at the hands of Sitting Bull's warriors in July 1876. In a concluding, one-hundred-page section entitled "The Last Stand as Ideological Object, 1876–1890," Slotkin demonstrates the systematic distortion of this event in the press, which avoided mention of the connection between Custer's expedition into the Black Hills and the development of Cooke's Northern Pacific land grants.

At the very end Slotkin draws a parallel between Custer's defeat and the fate of Mark Twain's Connecticut Yankee. This is an original and valuable suggestion, but it is marred by a defect that is noticeable elsewhere in the book. Slotkin's thesis raises philosophical issues that have been under debate for several decades now in such books as *The Social Construction of Reality* by Peter L. Berger and Thomas Luckman (which he cites). A writer who ventures into this minefield incurs a special obligation to make himself clear, and this Slotkin fails to do. What exactly is the relation he means to assert between "the Custer myth" and Mark Twain's ambiguous resolution of his fable? Even Slotkin's title, from Whitman's "Death-Sonnet for Custer," which he quotes as an epigraph, is far from clear in relation to the book as a whole. One can admire Slotkin's prodigious industry, his intellectual fertility, and the fresh perspective from which he views American popular culture, but one must wish at the same time that such basic theoretical difficulties were explicitly confronted.

HENRY NASH SMITH
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DAVID BENSMAN. *The Practice of Solidarity: American Hat Finishers in the Nineteenth Century.* (Working Class in American History.) Urbana: University of Illinois Press. 1985. Pp. xxi, 240. $22.50.

The "new" labor history, influenced by E. P. Thompson's work first published over twenty years ago, emphasizes the importance of human agency over economic determinism. It focuses on culture and community rather than on trade unions and other institutions. David Bensman's book is unusual because, though concerned with the history of an institution, the United Hatters of North America, it is influenced by the concerns of the new labor historians. It describes the values of the early artisan hatters: their commitment to job control, irregularity of work patterns, itinerancy, and maintenance of group discipline and solidarity. Bensman argues that this early solidarity was based on local autonomy and craft, not class, loyalty. This early parochial solidarity changed as economic and political developments led hat finishers to adjust both their culture and their union.

Bensman's book is about the movement of hatters from isolated craft and parochial solidarity to class solidarity. In the period 1800–95 changes in the industry forced a struggle to maintain traditions and culture. That struggle, in turn, changed the culture and traditions it was designed to protect. To fight prison labor, the finishers united with other unions and with manufacturers and lost some of their localism. In the conflict with the increasingly powerful manufacturers, the hatters were forced to give up their local autonomy—the key to their traditional democratic culture—to an increasingly centralized national union.

Bensman argues that despite diverse backgrounds the hatters were socialized through apprenticeship to a culture that stressed pride in labor and independence from the authority of the boss. A competitive market and severe depressions in the trade forced manufacturers to attack the traditions of the hatters. In struggling to thwart this attack, the hatters were forced to make several adaptations that transformed their culture. A new collectivism, in the form of an amalgamated union of all the hatting crafts joined to the national American Federation of Labor, replaced their parochial craft-consciousness and craft autonomy.

By combining some of the insights and strengths of the new labor history with traditional institutional labor history, Bensman has written a solid, informative book, but not one without problems. He fails to define the term "culture," which he uses in a vague fashion, and to consider the relationship between class and culture. Also, in discussing the movement from local autonomy to national business unionism, Bensman's analysis is from the national union's

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perspective. He should have considered the perspective of the locals, for which the struggle may have involved democratic unionism as opposed to business unionism, class struggle as opposed to institutional existence, rather than centralism versus parochialism. Bensman compares his hatters with other workers and other unions. Relating the economic struture of the hat industry to that of others would have strengthened his analysis.

JOHN T. CUMBLER
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JOHN S. GARNER. *The Model Company Town: Urban Design through Private Enterprise in Nineteenth-Century New England.* Amherst: University of Massachusetts Press. 1984. Pp. xiv, 288. $22.50.

Restraint, exploitation, and repression are often associated with company towns. Yet, according to John S. Garner, not all company towns deserve blanket condemnation. Many of them, especially those founded in rural New England during the nineteenth century, "embodied positive advances in planning and architecture" and "served as a favorable alternative to other types of industrial settlements" (p. xi). Some manufacturers created model industrial villages where they combined successfully the needs of industry with the requirements of labor. Part 1 of Garner's study surveys broadly the design and development strategies of five model industrial communities: South Manchester, Connecticut; Peace Dale, Rhode Island; Fairbanks Village, Vermont; and Ludlow and Hopedale, Massachusetts. Part 2 examines one of these villages, Hopedale, more critically.

Established during the 1840s as an experiment in Christian socialism, Hopedale declined in the following decade and was purchased by Ebenezer Draper and George Draper, manufacturers of textile machinery. After development passed to a single family, factories, homes, public buildings, and recreational facilities were designed and constructed to serve the Draper Manufacturing Company and its work force. Leaving little to chance or to individual initiative, the Drapers created a "model" industrial community. Toward the close of the nineteenth century, Hopedale received national and international praise for its town design, decent company houses, and lovely landscape with tree-lined streets, sports field, and extensive park system.

The Drapers believed that such surroundings attracted the best workers. Laborers exchanged their independence for relative economic and social security. Harmonious labor-management relations continued throughout the nineteenth century. The apparent claim, however, collapsed in 1913 when the International Workers of the World (IWW) organized and led a four-month strike. The Drapers defeated the strike but thereby shattered their image as benevolent employers and architects of the model community.

By spotlighting the rise of the small, single-enterprise, model company town, by adopting a limited comparative perspective that places American efforts within a broad European context, and by emphasizing the influence these model towns had on subsequent urban design and development, Garner's book enriches our understanding of the diversity and complexity of early American industrialization. As with any work, however, there are shortcomings. Garner's conclusions sometimes go beyond the evidence presented. His discussion of labor-management relations is a case in point. Although Garner argues that conditions found in Hopedale helped stabilize the relationship between employer and employee, he does not explain adequately why the IWW was successful in 1913 or what disrupted Hopedale's apparent harmony. Workers remain amorphous figures, and the undercurrents of anger and dissatisfaction that motivated the 1913 strike remain obscure.

BARBARA M. TUCKER
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FRANCIS PAUL PRUCHA. *The Great Father: The United States Government and the American Indians.* In two volumes. Lincoln: University of Nebraska Press. 1984. Pp. xxxii, 608; 610–1302.

This book stands as convincing testimony to the determination of Francis Paul Prucha to tell the whole story of the formation and implementation of government Indian policy. Over a professional career that has spanned some twenty-five years, Prucha has written and edited numerous books and articles examining the complex issues affecting relations between the United States government and the native inhabitants of America. This two-volume work contains the heart of those studies but adds fresh material, especially for the colonial era and for the twentieth century. Although the work brings together much of Prucha's previous writing, the seams are well hidden. It is a unified and coherent scholarly study that deserves to be read on its own terms.

Prucha's style has never been overtly interpretive. He has his point of view, of course, but his method has been to reveal the meaning of the formal relations between whites and Indians through a painstaking reconstruction of government policy. With the full sweep of the story in one work the interpretation is no less integral, but it does loom larger than in the more concentrated versions of his work.

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Prucha sees continuity and paternalism in government policy toward the native Americans. From the earliest stages of the new government's dealings with the tribes, when Henry Knox formulated the policies of the Washington administration, to the most recent efforts to satisfy the demands of a rejuvenated native people, the government has been consistent in its assumption of paternal jurisdiction over the Indians. Even in the later years, when the government recognized a residual native sovereignty, there could be no doubt that the federal authorities remained the ultimate arbiters of Indian life. Prucha is, of course, mindful of the vicissitudes and ironies that lie beneath this general conception. Through the first volume, which stretches to the 1880s, he demonstrates that, though divided on many particular issues, policy makers were united in the determination that the native people should abandon their "savage" ways and become an integrated part of the larger American society. The policy culminated in the Dawes Severalty Act of 1887 that sought to separate individual Indians from traditional societies and establish them as property-owning citizens. The consequences, predicted by many knowledgeable people, became evident almost immediately. Many of the native people who accepted separate ownership of land soon lost their property precisely because they were not ready to be fully integrated into the white man's society. The policy was soon abandoned, but it was not until the New Deal that John Collier formally replaced it with a policy designed to preserve tribal independence. With the close of the Second World War and through the Eisenhower years, separate ownership enjoyed a revival, this time described as "termination." The policy worked no better in its second incarnation and has in recent years been replaced by a further extension of the New Deal: the protection of tribal sovereignty by the government.

Prucha notes the ironies inherent in government policy. Through the early years the government's objective had been straightforward enough: the Indian was to cease being an Indian and become part of the white man's world, a policy that would in effect be self-liquidating. In time there would be no need for an Indian policy. But, despite the government's efforts and the many changes that occurred in native life, the Indians persisted and they remained Indian in character. And the Indian office never worked itself out of a job. Eventually the native people became wards of the government. Collier's determination to protect native independence also did not evade unintended consequences. In the name of independence, government policy became more invasive and the native people more subject to shifts in bureaucratic direction. More recently the concept of the government as trustee for the native people has come to be interpreted as the protection of the tribal sovereignty, "a seemingly inescapable paradox" (p. 1086). The wish to preserve native independence has led to "the great Father redivivus in pervasive form" (p. 1206).

Prucha is not unmindful that federal policy (or at least the action of those who implemented that policy) has occasionally done injury to the native people. He refers, for example, to the "injustice involved in the removal" of the southern tribes (p. 241) and later to the "injustices in the government's Indian system" (p. 469). But the injuries and injustices, it seems, occurred despite the good intentions of the formulators of government policy. The federal authorities always had in mind a greater good for the native people that might entail some immediate inconvenience but would in the long run redound to their benefit. And yet Prucha is not on the whole satisfied with the earlier policy designed to dissociate the Indians from their traditional ways and incorporate a cultureless shell into the white man's world. He sees the policy as unfortunate but necessary. He finds the Collier approach more acceptable, though marred by the arbitrariness and paternalism typical of reform in general. The point might well be made, though Prucha does not make it, that no policy has ever been formulated that would serve the interests of both Indians and whites. And it might be added that whether any such policy could be formulated is unclear. Early efforts rested on a profound disrespect for the integrity of native culture. More recent efforts have corrected that fault but have instead ensnared the Indian people in a web of bureaucracy, which, though it purports to be beneficent, generates endless controversy and discontent.

The picture that emerges from this book is for the most part unedifying, not because of the fraud and violence that often characterized relations between the two peoples but because of the futility and destructiveness that often arise from policies designed to accomplish great ends. *The Great Father* is a monument to the historical craft that should serve a generation as the basic source for the story of American Indian policy.

BERNARD W. SHEEHAN
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GLENDA RILEY. *Women and Indians on the Frontier, 1825–1915*. Albuquerque: University of New Mexico Press. 1984. Pp. xvi, 336. Cloth $24.95, paper $12.95.

In this impressive study Glenda Riley sets out to explore the relationship between two nineteenth-century minority groups: women and Indians. As she points out in the introduction, her primary objectives are to "examine the ways in which

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frontierswomen reacted to their initial contact and subsequent relations with American Indians" (p. xvi) and how and why pioneer women altered their own self-image and their views of American Indians as a result of such contacts. Obviously, Riley has set herself a difficult task, but she generally succeeds admirably.

Using examples from history and literature supplemented by period photographs and illustrations, Riley examines both the American and European influences that shaped women's views of themselves and the various minority cultures with which they came in contact. She also explores, in some depth, the images and myths regarding women and Indians that "impaired their interaction on the frontier and prevented generations of observers from perceiving its real nature" (p. 252). She also points out that nineteenth-century prejudicial images have been perpetuated to a large extent in the twentieth century in "both myth and media" (p. 252).

Having examined the myths, Riley then uses women's diaries, journals, reminiscences, and letters to challenge these stereotypic portraits of what constituted "real Indians" and "true women" and shows how women changed or discarded their earlier opinions in light of their experiences and in a way that men seldom did. On these points she concludes, "While many argue that white women often shared the prejudices of the men with whom they lived, the major finding of this study is that women's perceptions of minority groups were modified by their perceptions of themselves and their distinct place in American society" (p. xiii).

Although Riley's emphasis is on women's changing attitudes toward American Indians, the author also explains some of the reasons why she believes women's attitudes toward Mormons and Mexicans did not change following contact with these people. And, although the book focuses on women rather than on Indians, it provides an excellent synthesis of recent interpretations of Indian-white relations, which will be of interest to students of American Indians. Whether one agrees with all of Riley's interpretations (and this reviewer does not), her book provides an excellent model for future studies of cultures in conflict and transition.

SANDRA L. MYRES
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NICOLE HAHN RAFTER. *Partial Justice: Women in State Prisons, 1800–1935*. Boston: Northeastern University Press. 1985. Pp. xxvi, 269. $22.95.

Nicole Hahn Rafter traces the incarceration of women in America from the early nineteenth century until the mid-1930s. Other recent historians of penal institutions for women (especially Estelle Freedman and Barbara Brenzel) have intensively analyzed single institutions. By contrast, Rafter, whose focus is national, pays special attention to regional variations, which she highlights with brief case studies of five institutions in New York, Ohio, and Tennessee. These institutional portraits include a description of inmate demography based on a sample of over forty-six hundred individual case records.

In the nineteenth and early twentieth centuries, women's prisons approximated two quite different models. The first, which Rafter calls custodial, resulted from the gradual separation of women (always a small minority) from male prisoners in the new nineteenth-century penitentiaries. The other, which developed primarily after 1870, was the reformatory. The ideal reformatory accepted only young, potentially redeemable women convicted of misdemeanors (women felons went to the female branches of custodial prisons). Few institutions, however, ever completely matched the reformatory template, and, after 1935, the distinction between the two types faded. In the end, not surprisingly, the custodial model triumphed.

Rafter's title, *Partial Justice*, signals the ironic, often unfortunate, consequences of the new women's prisons and reformatories. Without doubt, women prisoners were treated brutally in predominantly male prisons. Nonetheless, even though "prison administrators often reacted with greater leniency toward women. . . , this partiality . . . did not produce better treatment," and in both custodial prisons and reformatories women "encountered conditions that were as unpleasant as those of men, though in different ways" (p. xxiv). In fact, gender discrimination began before women were incarcerated because they were convicted for behavior condoned in men. Key agencies in the late nineteenth-century war against prostitution and sexuality, "reformatories extended government control over working-class women not previously vulnerable to state punishment" and used the power of the state to "'correct' women for moral offenses for which adult men" were not imprisoned (p. 158).

As she delineates the complex connections between gender, race, class, and criminal justice, Rafter shows that most women offenders came from the margins of society. A disproportionate number of these women were unmarried, and they seemed especially vulnerable because they lacked families. They were, of course, almost always from the working class, and, particularly in the South, they were largely black. Their story is a dismal tale of social control dressed up as reform, of exploitation dignified as work discipline, of the deprivation of civil liberties disguised as progressive innovation, and of gender and race discrimination institutionalized as

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justice. Unfortunately, as Rafter argues in her conclusion, remarkably little has changed in women's prisons since 1935, and there is no reason to hope for much real reform in the near future.

This book has some problems: it understates the role of antebellum reformatories; it misses the important links between women's prisons and the social welfare system; its demographic analysis (its weakest aspect) could have been strengthened through the use of the manuscripts of the special schedules of the 1880 United States census and through multivariate analysis of key dependent variables; some comparative discussion of city and county jails with state prisons would have been illuminating; and in places the book's organization is a bit repetitious. Nonetheless, this study is a convincingly argued, well-researched, and strikingly illuminating, if depressing, addition to the literatures of gender, social institutions, and criminal justice.

MICHAEL B. KATZ
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NANCY A. HEWITT. *Women's Activism and Social Change: Rochester, New York, 1822–1872*. Ithaca, N.Y.: Cornell University Press. 1984. Pp. 281. $29.95.

Nancy A. Hewitt's well-focused study illuminates the public roles of nineteenth-century Rochester women in the cause of social change. She challenges the orthodox view that revivalism stimulated white middle-class women to extend gradually their sphere from charity to moral reform and ultimately to suffrage. Hewitt identifies three distinct groups of voluntary women—"benevolent" women, "perfectionists," and "ultras"—tracing their activities, organizations, and ideologies. Although all were white, Protestant, and roughly middle-class, they came from different regions and strata and retained distinctive qualities throughout the period.

Benevolents, reformers, and agitators collided, occasionally colluded, and ultimately stimulated one another to produce a range of social services and reform activities, from the benevolents' Orphan Asylum and Female Charity Society through the perfectionists' Industrial School, Home for Friendless and Virtuous Females, Ladies Anti-Slavery Society and Temperance Union to the ultras' Western New York Anti-Slavery Society, the Working Women's Protective Union, and National Women's Suffrage Society.

Benevolent women, mothers, wives, and daughters of Rochester's elite began their work in the 1820s and shaped the community's dominant perspective on women's proper public role. They directed the city's major social welfare institutions for fifty years, volunteering as administrators and district visitors, and blending social control and social welfare. Believing that "the poor are always with us," benevolent women discreetly entered the public sphere for long careers as the delegates of their powerful families.

Perfectionist women, initially fired up by revivals in the 1830s, were convinced that America could be purged of intemperance, slavery, and prostitution and that their efforts would hasten the millennium. As the fires dampened, these women moved to more conservative positions, often joining forces with benevolents and frequently becoming emboldened by ultra agitations. While conservatively believing in a clear distinction between worthy and unworthy poor, perfectionists radically advocated an unsegregated orphanage and prayed for the eventual abolition of slavery. These women's moral landscape changed with the rising fortunes of male kin who had by mid-century climbed to comfortable positions through new commercial ventures. Perfectionist men and women shared similar values but labored in separate spheres.

The ultras, or radical women, who often came from agrarian, Quaker families, joined worldly activism in Rochester during the 1840s. Less affluent than the other two groups, ultras and their kin lived on the outskirts of the newly industrializing city. Unlike the other two groups, radical women had no access to power through male kin; men and women shared both values and labor in their efforts to prosper, and they regarded blacks and workers "not so much as victims to be aided as coworkers to be organized" (p. 236). Susan B. Anthony, Frederick Douglass, and Sojourner Truth all labored for the cause in Rochester.

Hewitt makes good use of earlier works and links regional economic and social history to women's reform activities. Along with considerable information on individuals and groups, she also provides twenty-two tables detailing demographic data on all three groups. Her work is a model for future community studies and a welcome addition to women's history.

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WILLIAM H. PEASE and JANE H. PEASE. *The Web of Progess: Private Values and Public Styles in Boston and Charleston, 1828–1843*. New York: Oxford University Press. 1985. Pp. xiv, 334. $29.95.

The content of this book successfully establishes what its title and topical organization presuppose—the comparability of Boston and Charleston during the Jacksonian period. From this base William H. Pease and Jane H. Pease are able to explain with

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precision the significance of the differences they find between the cities. Much of their account compares municipal institutions and services, educational, religious, and political practices, responses to social change and natural disasters, and styles of daily life, especially among the elite, for whom the record is most full. In all of this, the Peases supply elaborate, digested detail. But their attention never strays for long from economic institutions and development. Both Boston and Charleston were long-established seaports where commercial activities employed about one-third of the work force. Both cities alternately prospered and suffered through the vicissitudes of the Jacksonian political economy, although Boston prospered and grew more—and suffered less—than Charleston did. Although both cities held similar aspirations for unbounded prosperity through commerce, some of Charleston's social priorities impeded its economic goals, whereas in Boston these were mutually supportive.

In Boston, natural and cultural influences converged to clear away most obstacles to "the pragmatic pursuit of private profit" (p. 40). Boston's mercantile houses did not have to compete (as Charleston's did) with agriculture for the resources of the upper classes. The textile and other manufactures that arose in its densely populated hinterland invigorated the city's commerce. Prosperous merchants grew wealthier by investing in these concerns and in the railroads linking them to the city. Municipal politics and functions readily accommodated themselves to the needs of commerce.

In Charleston, however, the interests of often-absent slaveholding planters of cotton and rice dominated almost every aspect of public life. The city's economy was held hostage to the fluctuating price of cotton and, in the long run, to deteriorating soil, which drove planters westward and generated commercial rivals in New Orleans and Mobile. The specialized, defensive political interests of planters (the prolonged distraction of the tariff controversy supplies only the most dramatic example of this), and the local rivalries that implemented them, increasingly separated Charleston from the national political course. Planter culture highly valued leisure, reliance on personal ties in public matters, and civic volunteerism; it placed a lower value on practical skills, entrepreneurial zeal, and the professionalization of urban services that accompanied modernization in Boston. Accordingly, utilitarian education lagged, and skilled white labor drifted away. Many projects necessary to make the city prosper were undertaken by volunteers or outsiders. Several ambitious modernization ventures that Charlestonians did plan, particularly for railroads, fell victim to their own grandiosity. Cultural and economic conditions converged to isolate Charleston as a commercial backwater whose needs were supplied—and staples transported—by Yankees.

Many of the themes the Peases develop, as they make clear, have been developed before. The great merit of their book is that it alternately substantiates and qualifies, while it everywhere deepens, a story its likely audience will presume already to know: how, despite their similar aspirations, Boston helped spin the "web of progress" and Charleston entangled itself on the edge of it.

GEORGE B. FORGIE
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HENRY C. BINFORD. *The First Suburbs: Residential Communities on the Boston Periphery, 1815–1860*. Chicago: University of Chicago Press. 1985. Pp. xiv, 304. $25.00.

The first law of history is that everything happened before you think it did. The sociologists "discovered" suburbs in the 1950s; Sam Bass Warner's seminal *Streetcar Suburbs* (1962) later demonstrated that, because of the revolution in mass transportation, inquiry into the development of suburbs must extend back into the late nineteenth century; now Henry C. Binford demonstrates that this process was evident in Boston between 1820 and 1860. The first entry for the term "suburb" in the *Shorter Oxford English Dictionary* is in the 1665 edition, but, in general, it is correct to state that historians have arrived at the serious study of suburbs by slow freight.

To attack the problem of New England suburbanization, Binford rolls out the heavy artillery of contemporary quantitative techniques. There are twenty-six tables, thirty-eight pages of notes, and a fourteen-page bibliography. And the introduction leads us to believe we are in for a brisk commute to pre-Civil War America: "Almost no one . . . has examined community development in the unique setting afforded by ante-bellum suburbs" (p. 8). Competing theories are dismissed, although some are described in a way that is often hard to recognize. Most of these views, the author asserts, "unintentionally trivialize suburban history" (p. 228), and he adds that "this work provides a corrective" to "city-centered views" of suburbanization (p. 228). The evidence for these claims lies in a close study of four Boston suburbs: Somerville, old Cambridge, Cambridgeport, and East Cambridge, all of which began as "fringe villages" in 1820 and became suburbs by 1860.

What would an urban historian expect to find in such an enterprise? The early settlements would be somewhat rural, with agriculture directed in part to an urban market; daily contacts with the nearby metropolis would be erratic and transportation uncertain; some manufacturing, directed especially

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toward prospective city sales, would develop; a small number of the wealthy would have closer and more intensive ties to Boston; and transportation facilities would be primitive. Over the decades, roads and bridges would make the city more accessible; immigrant newcomers would encourage a movement of the middle class and the affluent beyond city limits; the mounting numbers of suburbanites would require urban services, so local government would have to build roads, sewers, water mains, and schools and develop institutionalized government. The transition from village to suburb would create tensions, especially between the original residents and the newcomers. All of this would be fueled by the railroad, the omnibus, and the horsedrawn railroad. Over four decades a largely, though not exclusively, residential community in its modern outlines would emerge.

That is what happened, not precisely but generally. Binford, however, trumpets this process with an air of discovery. It was, in fact, the suburban experience of nearly every substantial city. Binford places too much theoretical baggage on too narrow a historical platform. He would be more persuasive if he had also examined comparable places, such as New York, Philadelphia, or Cincinnati. But we should be grateful for a careful, detailed study, intelligently organized and clearly written, that traces the development of suburban communities on the edge of one of the country's most important cities.

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AMY BRIDGES. *A City in the Republic: Antebellum New York and the Origins of Machine Politics*. New York: Cambridge University Press. 1984. Pp. xi, 210. $29.95.

In this original contribution to American political history, Amy Bridges analyzes why conflict generated by the spread of industrial society during the first half of the nineteenth century produced a politics of bosses, machines, and multiclass parties that superseded the first labor parties such as the Workingmen of the 1820s. In her study, as rich in ideas as it is economical in the exposition of argument, Bridges explores how the existence of nearly universal adult, white male suffrage in the early stages of industrialism profoundly affected the American experience. Possessing the ballot and a rich tradition of artisan political participation, American wage earners were readily integrated into the dominant civic culture and political system. They spoke the language of the middle class and placed their notions of moral economy within an American republican framework. But their adherence to republican values left workers vulnerable to incorporation into the major parties, just as the spread of the market economy undermined artisans' independence. Finally, the common school and temperance movements offered new strategies for realizing artisan ideals of achieving independence that tended to integrate some wage earners into middle-class politics. When large-scale immigration added greater ethnic and religious diversity to New York City's electorate, nativism further fractured the working classes but not just along ethnocultural lines, which tended to coincide with craft sectionalism that disposed some workers to self-help and mutualist strategies endangered by the immigrant tide.

Until the 1850s neither Whigs nor Democrats clearly dominated politics in New York City. Locofoco Democracy gave the Jacksonians the edge in the 1830s, but ethnocultural conflict boosted Whig fortunes in the 1840s. By the 1850s, however, the rising tide of Irish, Germans, and Catholics gave the Democrats the advantage. The Democrats recognized labor spokesmen, and one faction in the party built its fortunes on workingmen's clubs and on a program of state intervention to ease hard times in the 1850s, challenging laissez-faire pieties. Boss Tweed combined lavish spending that created jobs with the defense of immigrant and Nothingarian culture to become the prototypical boss. Bosses displaced prominent businessmen who had dominated public office, but these elements did not abandon politics; rather, they retreated behind the doors of power. The boss directed class and ethnic conflict into orderly channels and mediated between those who possessed most of the votes and those who possessed most of the wealth. The cost of mediation—mounting city expenditures and high taxes—generated "reform" movements led by the city's entrepreneurs who sought to regain direct control over government. The irony is that the city's capitalists did not appreciate the vital contribution boss government made to accommodating workers to industrial society and deflecting more radical challenges from below.

Bridges finds crucial distinctions in the politics of workers engaged in the old artisan trades, not yet proletarianized, who were susceptible to mutualism and nativism, whereas the industrialized trades were more prone to gravitate to the class appeals of the Democrats. Likewise, the fresh treatment of ethnicity and class for once joins these variables in a subtle interrelationship. Useful comparisons between New York and other cities point up the importance of commerce and the export trade in making the antitariff arguments of the Democrats compelling to New York workers and capitalists but not in cities oriented to domestic markets.

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Certain issues, however, require further consideration. The boss was not unique to big cities. Bosses like Matthew Quay with statewide machines suggest that the city boss was a special case of a general type. The analysis, therefore, needs to move beyond the framework of city politics. Bridges's complex delineations of fractions within the social structure rests on an inconclusive statistical base that further research and data manipulation might modify or strengthen. There is little discussion of national politics, especially from the 1840s; the slavery controversy goes virtually unmentioned. Finally, the overall spareness and selectivity of research in original sources falls short of the historian's expectations.

The strength of this work is the political scientist's unobtrusive use of theory, a comparative sensibility, and the clarity with which the questions were framed. These virtues may not appeal to all historians, who prefer thick description, immersion in sources, and a closer adherence to traditional narrative. Bridges's holistic approach—with its focus on the power system, its careful delineation of differences among workers, and its exploration of how they voted—goes far to explain why neither socialism nor a labor party found firm root in the United States. A paperback edition is in order.

PAUL GOODMAN
University of California, Davis

THOMAS BROWN. *Politics and Statesmanship: Essays on the American Whig Party*. New York: Columbia University Press. 1985. Pp. 330. $32.50.

Thomas Brown defines Whig statesmanship, in contrast to Jacksonian particularism, as a commitment to promote the common good. Differing also from the stance of earlier Federalists, Whig statesmanship was not an ideology directed at a small elite but one that assumed that American voters generally "were capable of reason, moderation, and self control" in pursuit of principle (p. 10). While he questions that "in the highly politicized world of mid-nineteenth century America" the electorate "made a totally conscious, politically sophisticated judgment on each issue and candidate presented" (p. 14), he doubts, too, that such matters were "as irrelevant to the concerns of the . . . voting public as some of the literature might lead one to believe" (p. 13). He therefore seeks to delineate the adjustments through "political rhetoric" by which party leaders cultivated "a shared outlook" and a basis "for common action" (p. 14).

Brown develops his theme in chapters detailing the views promulgated by Daniel Webster, William Seward, Henry Clay, and Alexander Stephens. His focus on the translation, rather than the delineation, of the ideology is evident in his selection of these spokesmen and in the range of coverage provided on them. The inflexible John Quincy Adams, who himself claimed paternity of the American system in 1833 and to whom Clay consigned the defense of the Whig program in 1842, is scarcely mentioned as a leader after 1829. Of the essays presented, that on Webster is the best developed. The one on Clay is conceptually sound but limited in perspective and inaccurate in detail. The sketches on Seward and Stephens are even more abbreviated—in the case of Stephens perhaps because he professed adherence to the Whig program so briefly. The leaders appear here primarily to illustrate facets of the Whig argument under various aspects of political challenge.

This integration of ideology and historical events is the principal value of Brown's study. Organizationally it is strongest in generalized chapters like "The Rhetoric of Party Formation" in the North and the interpretaton of Whig goals in the South. Brown sees the birth of the Whig party as a consolidation of diverse factions brought together successfully only when events of Jackson's administration attracted a popular base under the rallying cry of "executive usurpation." Less convincingly, he accords to William Henry Harrison a position of significance in that development ranging beyond the emotionalism of the "Log Cabin" campaign.

The analysis of southern Whiggery explores the basis on which staunch defenders of the "peculiar institution" found grounds for union as Whigs against Jacksonian Democracy. For a time the statesmanship of compromise and the hope for more dynamic economic growth afforded important rationalizations. Brown contends that the programs of the American system had become irrelevant in the dialectic of parties after the mid-1840s and even the statesmanship of compromise was impracticable by 1850. Limiting his considerations to southern Whigs of the states' rights persuasion, he offers little insight into the role of those who, like John Jordan Crittenden or John Bell, sought to preserve the concept of nationalism. One wonders why the Republicans of 1860 adopted anything more than slavery planks in their platform strategy.

MARY W. M. HARGREAVES
University of Kentucky

PAUL CONSTANTINE PAPPAS. *The United States and the Greek War for Independence, 1821–1828*. (East European Monographs, number 172.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. xvi, 190. $20.00.

From the French Revolution to the present-day support of Israel, the struggle between heart and

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head (or, in John Adams's words, between "feelings" and "reasonings") has complicated the conduct of American foreign relations. In the early nineteenth century, excitement over the Latin American revolutions had not died down when the Greek war for independence generated much public concern, setting the friends of liberty against advocates of trade with the Ottoman empire and of abstention from Old World politics as well as pitting those who supported financial aid for the Greeks against those who thought that the American domestic poor should come first. Inevitably, then, much of the Greek story has a contemporary ring: when the supporters of nonintervention defeated Daniel Webster's attempt to commit the government to support of the revolutionaries, a motion to extend "strictly humanitarian" aid was put forward, also to fail.

To the literature on this recently neglected subject (although hardly, as the author contends, "for fifty years entirely neglected" [p. vii]), this book by Paul Constantine Pappas brings two contributions: an extensive (but unindexed) survey of forty-eight American newspapers as a guide to public opinion and, potentially much more important, the first serious use of Greek sources in an English-language treatment. Regrettably, Pappas's diligence in research is not matched by skill in presentation. The chronology—whether Greek, Ottoman, European, or American—is obscured by parallel chapters providing unintegrated descriptions of the Greek revolt, American sympathy, efforts for aid and recognition, debates in Congress, the dispatch of agents (on which Pappas includes useful new detail), the construction of frigates for the Greeks, and the activities of American Philhellenes in Greece. Despite the use of Greek sources, the nature, location, activities, and infighting of the revolutionary government remain as cloudy as ever.

The book has other general problems. Some good older secondary works are used to the exclusion of newer better ones. The style is infelicitous, as in the frequent use of "to pressure" as an undifferentiated substitute for "to persuade," "to press," "to push," "to urge," and "to force." There are errors of detail, as in the general transmutation of ministers into ambassadors. Greek names are spelled in various ways; French "naissaux" are defined as "double-panked" frigates; and a half-page of endnotes is printed twice, obliterating on its second appearance all bibliographical entries from "Nathans" to "Raisis."

Those with some expertise on these matters may find certain details of interest. For more general readers the older treatments remain superior.

JAMES A. FIELD, JR.
Swarthmore College

RICHARD L. BUSHMAN. *Joseph Smith and the Beginnings of Mormonism*. Urbana: University of Illinois Press. 1984. Pp. 262.

In spite of Richard L. Bushman's careful research and lucid presentation, this fine book may ultimately fail to satisfy the two communities for which it was written: Mormons interested in their own history and historians intrigued by the Mormons. The book itself is a rich narrative. It describes authoritatively the efforts of Joseph Smith's parents to earn a living in Vermont and New Hampshire during the years surrounding his birth in 1805; their decision to move to upstate New York, near Palmyra, in 1816; Joseph's "first vision" in 1820; the discovery of the Book of Mormon in 1823 after a visitation from the angel Moroni; Joseph's sojourns in the Susquehanna valley, his involvement in treasure hunting, and the family's financial reversals during the mid-1820s; his marriage in 1827; the translation of the Book of Mormon in 1828 and its publication in 1830; the gathering of a new church in that same year; and, finally, the migration of Smith and his followers to Kirtland, Ohio, in 1831. Along the way Bushman offers brief but perceptive sketches of Smith's most important associates, converts, and critics, and he deftly outlines the historical landscape in which Smith moved.

The book testifies to a far-reaching mastery of sources and a disciplined search for authentic readings from difficult documents, especially the several versions of Lucy Mack Smith's *Biographical Sketches of Joseph Smith*. In setting out his own views, Bushman also sensitively employs the arguments of Smith's critics. A superb chapter on the Book of Mormon, for instance, catalogues the criticisms of Alexander Campbell (who thought Smith fabricated it from bits and pieces of his own biography in order to undercut Christianity), E. D. Howe and Philastus Hurlburt (who traced the book to a lost manuscript by Solomon Spalding), and twentieth-century scholars (who have returned to a secularized version of Campbell's environmental theory). Only after carefully taking the measure of these theories does Bushman return to his own analysis and conclude that converts to Mormonism "touched the book, and the realization came over them that God had spoken again" (p. 142).

As this judgment suggests, Bushman writes as a believing Mormon. At the same time, he bends over backward to avoid the sort of rationalistic historical apologetics that Mormons have often directed against the overweening skepticism of their critics. His method is rather "to relate events as the participants themselves experienced them" (p. 3). Mormons may find the resulting chronicle too "historical," especially when Bushman describes the prophet's periodic indecision, the apparent contra-

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dictions in different accounts of revelations to Smith, and the absence of non-Mormon verification for critical events in the founding of the new faith.

Historians, however, will miss the interpretive creativity that made Bushman's earlier work on Jonathan Edwards and the Connecticut Puritans so powerful. Bushman's psychological, demographic, social, and political interpretations may not have been the last word on their eighteenth-century subjects, but they pushed all students of the period to deeper levels of understanding. As he recounts the life of Smith, however, Bushman fends off similar interpretations that non-Mormons have offered for Smith's behavior, while leaving the field to explanations that the early Mormons provided for themselves. The result, though impeccable in research, nonetheless looks like an apology.

Historians who believe in supernatural realities, but not in those that Smith claimed to experience, will find this book instructive for efforts to write the story of their own religions. Secularists, who reject the possibility of divine revelation, will dispute Bushman's premise but admire his handiwork. Together, the non-Mormons will question whether Bushman provides sufficient justification for treating Smith's visions, the golden plates of the Book of Mormon, and the contents of that book itself as objective realities instead of as subjective experiences or skillful frauds. In the end this monograph is a masterful example for all historians—whether Mormons, believers in other faiths, or nonbelievers—both of how much can be done to write history across confessional boundaries and of how much such efforts still depend on questions of truth.

MARK A. NOLL
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LINDA KING NEWELL and VALEEN TIPPETTS AVERY. *Mormon Enigma: Emma Hale Smith; Prophet's Wife, "Elect Lady," Polygamy's Foe, 1804–1879*. Garden City, N.Y.: Doubleday. 1984. Pp. xiii, 394. $19.95.

At one level, this is a well-researched, objective treatment of a mid-nineteenth-century woman who was married to a prominent man. At another level, it is revisionist history of a special kind. Linda King Newell and Valeen Tippetts Avery deal sympathetically with Emma Hale Smith's loathing for polygamy and the tortured years in which she alternately tried to come to grips with the practice and vehemently opposed it. Although to the non-Mormon reader her position seems completely understandable in human and societal terms, the fact remains that to members of his church Joseph Smith is a prophet, who followed not his own will but God's commands. In rejecting her husband's decree—a serious offense for any American woman of the time—Emma was also guilty of refusing to carry out God's specific wish. The strength of the Mormon respect for the doctrine of continuous revelation, not the material itself, requires considerable argument about what often seem trivial points in order to rescue the reputation of Smith's first wife from a century of religious vilification and neglect.

Like many American women of her time, Emma was able to be more strong-minded as a widow than as a wife. For those concerned less with her role within the Mormon church than with the portrait of a woman facing almost incomprehensible strains, the most valuable parts of the book will be her letters, blessings, and recorded thoughts. The book is also an object lesson in the difficulties of a woman confronting patriarchal religion, even without the added problem of divine revelation. Since Emma remained a good and believing Mormon all her life, her courage and independence of mind on this issue, which feminists of her time and our own find as repugnant as did she, is particularly impressive. The non-Mormon historian would prefer to see more of an attempt to place Emma in the context of other women of her time, making use of the literature about nineteenth-century American women, women in American religion, and women in the American West.

The authors in conclusion quote one of Emma's female contemporaries who had changed her mind and decided that Emma, with all her faults, was a good woman to whom, she hoped, the Lord would be merciful. Whatever her prospects in the next life, Emma has found merciful and meticulous biographers in this one.

BARBARA WELTER
Hunter College and
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JAN SHIPPS. *Mormonism: The Story of a New Religious Tradition*. Urbana: University of Illinois Press. 1985. Pp. xviii, 211. $14.50.

This is not another history of that most American of America's indigenous religions. Indeed, if it is exterior or "linear history" that one seeks, Jan Shipps recommends several other books, not her own. Yet she presents a good deal of "linear history" here, fascinating in its detail, its revisionism, and its evidence of wide reading and up-to-the-moment observation. She even includes a fair amount of inside gossip concerning the Utah church, and it is the far larger group with headquarters in Salt Lake City, rather than the small Reorganized Church of Latter-day Saints, that drives her story. Mainly, however, this book intends to demonstrate the force

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of the subtitle, namely, that Mormonism is a genuinely new religious tradition. This is Shipps's thesis; it is her burden. Mormonism is as distinct from its origins, she argues, as Christianity is from Judaism.

To make that case, the author leans on cultural anthropologists and theoretical sociologists, on the social historians of early Christianity, and on the phenomenologists of the Eliade school. So candid is she about the sources of her analysis that analytical structure often interferes with the mythic "story" being told. With a touch of the new convert's enthusiasm for the history-of-religions theory, she invites the reader into her study to watch her work it all out. And she does work it all out, showing how, for example, early Mormonism really did recapitulate the patriarchal age of the Old Testament. For those pioneers, that age was neither metaphor nor symbol but a living experience in sacred time and place. Shipps argues so persuasively that one is virtually prepared to rechristen Mormonism as the Church of Abraham of Latter-day Saints. This, however, would be to go too far, for unmistakable Christian elements are present in Mormonism as well, explicitly so in the articles of faith, which begin, "We believe in God, the Eternal Father, and in His Son, Jesus Christ."

Yet Shipps will not allow us to regard these Latter-day Saints as just another of Protestantism's endless variations on the Christian religion or as merely another frontier manifestation of a restorationism trying to revive primitive Christianity. Mormonism, she insists, is something new: a new prophet, a new scripture, a new and uniquely radicalizing historical experience. Speaking of the Western world's religious fecundity, Shipps writes, "Of the cultic movements whose members accepted radically revised or fundamentally altered versions of the faith-stories regnant in their cultures, only Christianity and Mormonism are now full-scale religious traditions" (p. 50). To sustain this point, the author declares that the Mormon church does not share the European background of the rest of American Christianity. Apart from how this assertion might apply to Eastern Orthodoxy and black Christianity in America, one has other difficulties with this claim. The failure of the Book of Mormon to deal with the Middle Ages, the Renaissance, and the Reformation cannot mean that Joseph Smith, Oliver Cowdery, Orson Pratt, Sidney Rigdon, and Brigham Young were levitated out of a Europeanized nineteenth-century United States. Nor can Mormon scripture be wholly divorced from the King James Version of the Bible, which is simultaneously a creative force in, and product of, Western Europe. Moreover, even if we grant that pioneer Mormonism (1830–90) is unarguably unique, does that same uniqueness persist in modern Mormonism, that is, in Mormonism after the 1890 manifesto against polygamy and the 1896 admission of Utah to the union?

History, the story of the signs and wonders of the visible world, is a troublesome subject for Mormons. Controversy continues into the present, as more is gradually revealed of Joseph Smith's boyhood and the family's active role in the earliest years of the movement. Similarly, the history of Mormonism presents difficulties, or perhaps opportunities, for Shipps: although she clearly knows the "linear history" exceptionally well, she wishes to move above and beyond exterior developments to tell a story that is "as hermeneutical as it is historical" (p. x). Telling her story in such a way involves risks, but Shipps bravely and imaginatively takes them on. We may all be the richer for it.

EDWIN S. GAUSTAD
University of California, Riverside

ROBERT M. SENKEWICZ. *Vigilantes in Gold Rush San Francisco.* Stanford: Stanford University Press. 1985. Pp. ix, 272. $24.95.

For over one hundred thirty years, the famous San Francisco Vigilance Committee of 1856 has fascinated American historians. The most salient feature of this historical literature has been the plot theory, which alleges that a group of politicians and criminals had so degraded and misgoverned San Francisco that nothing but the strong medicine of vigilantism could cure the city of its ills. Since the Second World War, historians have systematically dismantled that theory, and Robert M. Senkewicz is the latest to join in this assault.

He is also the least effective. There is little original research or interpretation in this book, and the thesis differs only marginally from previous accounts. In rejecting the idea of political conspiracy, Senkewicz joins a small number of recent historians who propose counter-plot theories of San Francisco's collective violence. Senkewicz hypothesizes that the merchant-vigilantes sought to create an image of disorder that would persuade nervous eastern merchants to stop shipping goods to a vastly overstocked market, thereby increasing the prices of San Francisco merchandise. More important, the vigilantes hoped to seize power in the city by violence and to govern according to their own philosophy. That meant reducing both municipal spending and Irish Catholic political power. To Senkewicz, the excitement surrounding the formation of the 1856 committee provided a pretext for the seizure of power by a longstanding nonpartisan movement, which could find no other way to realize its cultural and class aims.

For those who fancy plots, this is a neat one, although the reader will not find it stated in sum-

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mary or coherent form anywhere in the book. The author's confusion is particularly evident in his attempt to explicate the vigilantes' motives: successively the motives were anti-foreign, anti-Irish, anti-Catholic, mercantile (a grand speculation in flour), and psychological (the need for a scapegoat); alternately, the merchants were driven by envy of politicians' status and success and a lust for power. None of these theses is implausible, and yet none is tested, carefully analyzed, or coherently related to the others in a clear statement of interpretation. For example, Senkewicz devotes an entire chapter to the "scapegoats," in which he tells of ruinous business practices that brought financial ruin to masses of merchants who, to explain their own failures, therefore needed to find scapegoats. Then he informs the reader, in the appendix of all places, that the vigilante leadership was drawn not from the ranks of these failed merchants at all but from the most successful sector of the mercantile class.

Perhaps the most convincing part of this confused book is the appendix, which is an able and scholarly discussion of the historiography of vigilantism in the city. This discussion and the body of the book demonstrate that, without fresh research, fresh sources, and fresh methodology, there really was no point in writing another book on the San Francisco Vigilance Committee of 1856.

ROGER W. LOTCHIN
University of North Carolina

JAMES LEE MCDONOUGH. *Chattanooga: A Death Grip on the Confederacy*. Knoxville: University of Tennessee Press. 1984. Pp. xviii, 298. $19.95.

LARRY L. DANIEL. *Cannoneers in Gray: The Field Artillery of the Army of Tennessee, 1861–1865*. University: University of Alabama Press. 1984. Pp. xii, 234. $19.95.

Interest is rising again in the military history of the Civil War, especially the war in the west. The two books under review represent traditional battle studies but with a fresh focus. They both are concerned with interesting aspects of the Confederate Army of Tennessee, and both are worthy contributions to Civil War history.

James Lee McDonough's *Chattanooga* is a detailed study of General Braxton Bragg's attack on that important communications center in September 1863 and especially of his ill-fated attempt to capture it by siege. McDonough's good book offers important information, drawn from a wide variety of northern and southern sources, on Bragg's curious quarrels with various generals as well as on Bragg's casual command posture. Moving beyond a clear analysis of personalities as factors in victory and defeat, McDonough restudies the entire Lookout Mountain–Missionary Ridge campaign in detail. And, despite the plethora of works on these battles, *Chattanooga* breaks fresh ground.

McDonough is a historian who believes in operational studies and does them well. Using unit histories, personal papers, memoirs, and such traditional sources as the *Official Records*, he reconstructs the action on both sides of the line. Careful personal reconnaissance of the fields leads him to new insights about the peculiar difficulties of attacking and defending the Confederate positions above Chattanooga. Paying particular attention to tactics—a topic often neglected by historians—McDonough concludes that bad advice on engineering problems, plus unfamiliarity with defensive fighting, led Bragg and some of his subordinates into serious errors of deployment, especially at Missionary Ridge. Rarely in Civil War studies has so convincing a tactical indictment been made.

Scrupulously detailing Union bravery and never minimizing the importance of luck combined with pluck, McDonough's account of Missionary Ridge shows the wages of incompetence and confusion. Harsh terrain, confused orders, awkward deployment, and poor artillery positioning all contributed to the panic that crazed Bragg's ranks as Grant's attack rolled upward toward the crest of the ridge. McDonough does not, however, neglect the problems inherent in Grant's situation.

McDonough's study is battle history expertly rendered and skillfully written. Only once does he allow himself to lapse into the eastern versus western theater argument (he is for the west). Beyond that pardonable plaint, he makes his case solidly and relentlessly demolishes all vestiges of Bragg's repute.

Just as *Chattanooga* stands as battle history with new insights, so, too, does Larry L. Daniel's *Cannoneers in Gray*. Daniel also uses diverse and skillfully detected sources and brings insight to a little-discussed facet of Confederate history. Artillery in the South's field armies has had scant notice since Jennings C. Wise's *Long Arm of Lee* (1915) and scattered other studies. A romantic haze still clings to the outgunned, outsupplied, and outranged rebel artillery and artillerists. Daniel dispels the haze and reveals tough men, devoted to their perilous careers; men who themselves shared some romantic illusions in the beginning but who learned as they fought. Southern resources never gave them an equal chance against their stalwart foemen. Gray gunners were also hampered by overt neglect from such generals as Albert Sidney Johnston, Bragg, and John B. Hood. These leaders either ignored or misunderstood artillery and did little to enhance the efficiency of what could have been a great asset. Daniel believes that only Joseph E. Johnston grasped the portent of his cannons; he worked hard

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to improve personnel, equipment, and organization.

In his study of men and guns and how they were used, Daniel writes an interesting blend of organizational and operational history. Always underranked, Confederate battery and battalion commanders never had the impact on strategic and tactical decisions they deserved. Equipped usually with twelve-pounder Napoleon guns, sometimes with imported or captured rifles, and sometimes with heavy ordnance, rebel gunners were best at close range; in the open they were often battered. Confederate ordnance works supplied reasonable numbers of guns and caissons and shells but never could match northern bounty. In organization, particularly, the Army of Tennessee's artillery suffered. Rarely did a general know how to use brigade, division, or corps artillery; rarely did the army's reserve artillery play a decisive role. As the war progressed, the artillery's role changed from heroic exchanges in the open to great barrages and severe counter-battery work. Few generals, among them Grant, Sherman, and Joseph E. Johnston, noted the change.

FRANK E. VANDIVER
Texas A&M University

GENE L. HOWARD. *Death at Cross Plains: An Alabama Reconstruction Tragedy*. University: University of Alabama Press. 1984. Pp. xiv, 151. $14.95.

Gene L. Howard presents a narrative account of one southern racial murder during Reconstruction. On July 10, 1870, blacks from Patona, Alabama, raided nearby Cross Plains to retaliate for the clubbing of a black man. On July 11, a white vigilante posse captured suspected raiders and began a "trial." That night, Ku Klux Klan members took the five suspects from custody and hanged them. One victim was William Luke, a white Canadian despised as a "nigger teacher" and accused of selling guns to blacks. A court of inquiry of the state government investigated the outrage, but white community solidarity hampered its work. The grand jury indicted no one for the lynching.

Howard gives readers a full history of a crime that has received brief mention in summaries of Klan violence. Even his thorough research, however, has not yielded many new sources on the specific events of July 10 and 11, for which he depends mainly on the report of Judge Thomas M. Peters of the court of inquiry and on references by witnesses in the congressional investigation of the Ku Klux Klan.

Howard does contribute much new information on the background and consequences of the tragedy. He brings to light the life and idealism of William Luke, who dedicated himself to the welfare of freedmen after a moral lapse ended his work as a minister in Canada. Howard graphically describes the local Reconstruction setting, in which racist antagonism focused on the benefits that the northern-controlled Selma, Rome, and Dalton railroad gave its black employees.

Howard's most intriguing source is oral tradition about Klan activity, chiefly provided by a descendant of a Klan leader. This testimony indicates that before the July 10 incidents, the Klan had already appointed an execution squad to kill Luke. Description of the squad members is so detailed as to approach identification, but the source apparently does not say what roles those individuals played in the later lynching. Howard does not provide a transcript of this oral testimony, and his citation methods do not clearly show its extent.

An interpretive problem concerns the relation between the vigilante posse and the Klan lynching. Howard concludes that the groups overlapped in membership but differed in leadership. Major Andrew Bailey, the posse leader, objected to the lynching, and prominent conservatives defended the posse, but not the lynching. The court of inquiry pointed out, however, that the posse itself was an unofficial and partisan group of white conservatives. Since the lynching cut short the posse's proceedings, one cannot know what their result would have been.

This book gives a view of Reconstruction life, and death, in microcosm. Howard tells the story with admiration for Luke and most Republicans, as well as with respect for moderate conservatives. Gary B. Mills, in introducing the book, takes a more conservative view of Reconstruction. Readers of all persuasions will appreciate this new light on the Cross Plains atrocity.

JACK P. MADDEX, JR.
University of Oregon

PETE DANIEL. *Breaking the Land: The Transformation of Cotton, Tobacco, and Rice Cultures since 1880*. Urbana: University of Illinois Press. 1985. Pp. xvi, 352. $22.50.

Pete Daniel has written an excellent study of southern agriculture in the past century. He traces the transformation of the cotton, flue-cured tobacco, and prairie rice cultures from labor-intensive to capital-intensive enterprises, from sharecropping to agribusiness. The forces behind this vast transition were mechanization and government policy. Drawing on an impressive variety of sources, Daniel describes these changes in broad strokes, but without losing sight of the human side of the story. His sympathies clearly lie with the masses of small farmers and tenants who lost out in the process of modernization.

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Cotton, tobacco, and rice underscore the diversity of southern agriculture. These cultures mechanized at different rates, and they reacted differently to federal programs. As early as the 1880s, rice growers began to mechanize, and transplanted midwesterners who had experience growing small grains played the leading role. Meanwhile, cotton and tobacco farmers remained incredibly backward. The Great Depression hit all three cultures hard, but the commodity programs of the Agricultural Adjustment Administration (AAA) operated independently with separate effects. The tobacco and rice programs produced little structural change in traditional tenure arrangements. In both cases, federal programs stabilized the existing owners and tenants while limiting opportunities for new farmers. The rice program awarded allotments to producers, including both tenants and landlords. If this approach had been applied to tobacco and cotton cultivation, Daniel suggests, it would have offered an alternative that might have changed the future of southern agriculture.

In contrast to the effects of the tobacco and rice programs, however, the effect of the AAA cotton program proved "complex, contradictory and revolutionary" (p. 182). The old cotton culture collapsed under the weight of both acreage reduction and mechanization. Government policy saved the large landlords but displaced cotton tenants and sharecroppers, literally clearing the way for capital-intensive agriculture. Many landlords took government checks and invested the money in tractors. The cotton South experienced its own enclosure movement, as small farmers and tenants were pushed off the land. The small number of protests, according to Daniel, shows how quietly small-scale cultivators left.

After World War II, all three cultures were poised on the brink of total mechanization. The introduction of the mechanical cotton picker and the manufacture of chemical herbicides enabled cotton growers to mechanize; tobacco growers followed in the 1970s. The sharecroppers soon vanished, and the transition to capital-intensive agriculture was complete. Did it have to happen this way? This is the question that hangs over Daniel's book. He knows that most small farmers and tenants lived lives of poverty and disease, but he is still nostalgic about the loss of a way of life. He does not, however, provide any reasonable grounds for thinking that small-scale agriculture could have been saved. No one has the solution to prevent farm failures and the decline in farm population. "The process of dispossession began with the eviction of sharecroppers," Daniel writes, "and it continues with the failure of millionaire farmers" (p. 291). But Daniel has brought together a vast amount of information and made an important contribution to our understanding of the rural South as it is today.

DONALD HOLLEY
University of Arkansas, Monticello

GILBERT C. FITE. *Cotton Fields No More: Southern Agriculture, 1865–1980*. Lexington: University Press of Kentucky. 1984. Pp. xiii, 273. Cloth $28.00, paper $10.00.

Agricultural historians have devoted more attention to the South than to any other region of the nation. The uniqueness of slavery as a labor system, the dominance first of tobacco and then of cotton in export markets, and the sharp contrasts between economic and social classes have offered opportunities for rewarding research. In 1933, Lewis C. Gray published his *History of Agriculture in the Southern United States to 1860*, the first and still the most comprehensive agricultural history of the region to the Civil War. During the half-century after Gray's volumes, no comparable study for the post–Civil War period appeared until 1984, when Gilbert C. Fite published his award-winning study.

Fite brings to this task a wide-ranging knowledge and many publications in the field of agricultural history. In recent years, through his research in the Richard B. Russel papers of the University of Georgia and in source materials in many other records depositories, Fite has acquired a probably unique understanding of the forces that brought about changes in southern agriculture. The result is a book that is destined to stand the tests of time.

In general, Fite follows traditional historical chronology and emphasizes three major periods of change: the development of the sharecropping system after the Civil War, the New Deal period and its impacts, and the post–World War II transformation of American agriculture, particularly as it affected the South. In each of these major periods of change, some farmers fared well. After the Civil War, some abandoned cotton and profited by producing fruit and vegetables for the growing urban markets. During the New Deal, the many farmers who were able to take advantage of the advice of county agents and the work being done in the state agricultural experiment stations survived hard times. After World War II, the tens of thousands of southern farmers who had access to capital and could enlarge and consolidate their holdings and mechanize their operations did well.

The problems of southern agriculture were not with these comparatively few farmers who were probably both able and fortunate but with those at the bottom of the agricultural ladder, who had no opportunities whatsoever to improve their condi-

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tions. Historians generally agree that sharecropping after the Civil War kept many farm workers underemployed in a system that despoiled the soil, promoted illiteracy and poverty, and encouraged a segregated social system.

The results of the New Deal farm programs lead to less agreement. Their objective was to preserve the traditional structure of American farming, but the programs left sharecroppers and small farmers in the same disadvantaged position in which they had been caught for generations. Indeed, it can be argued that the position of these agriculturalists worsened because many were forced off their farms. Programs to aid the poor helped only a few; many of the poor and mid-level-income farmers left the soil. As Fite writes, "No amount of agrarian thinking or rhetoric could result in policies that would tie people to small self-sufficient farms in an industrialized, modernized America" (p. 160).

Even greater changes took place after World War II, when farming became fully mechanized and every other aspect of production on the farm was improved as far as science and technology would permit. There was no longer a place for thousands of poor southern farmers who could not become a part of modern agriculture. These people, separated from the soil by technology, public policy, and economic change, struggled to find a place in American society. No matter what confronted them, however, those still on the farms were glad that the agriculture of hoe, mule, and cotton was gone forever.

Fite addresses these problems with the competence that he has shown in many previous books. He defines and sharpens the issues and provides us with the knowledge that we need to understand southern agriculture from the Civil War to 1980, enriching his account with details and examples that enhance our understanding. Fite has met a long-standing need in agricultural and regional history.

WAYNE D. RASMUSSEN
Department of Agriculture
Washington, D.C.

DON H. DOYLE. *Nashville in the New South, 1880–1930.* Knoxville: University of Tennessee Press. 1985. Pp. xvii, 288. $19.95.

In 1980, Nashville's Century III Commission invited Don H. Doyle to write a comprehensive and scholarly history of the city since its centennial celebration of 1880. The resulting study should help both professional historians and Nashvillians understand a city long known for its economic, political, and cultural schizophrenia.

The period from 1880 to 1930 was a critical one for Nashville as for other cities of the New South. Blessed with a large hinterland, good railroad connections, and the accumulation of wealth in the hands of shrewd, though conservative, investors, Nashville was a city of great ambitions—ambitions only temporarily blunted by the Panic of 1893. Wealth gained in commerce was invested in transportation, manufacturing, banking, and insurance—the latter two so significant that by the 1920s Nashvillians were referring to their city as the "Wall Street of the South." Although Nashville was never able to win control of the transportation facilities that served it and the growth of manufacturing was disappointing, the city could point with pride to an impressive central business district, a wealthy and cultured elite, and the highest per capita wealth of any city in Tennessee.

This era, however, was not without its problems and conflicts. Not far from the magnificent central business district were the "human sewers" of the black and white slums. The fragmented white business community reacted to these "dangerous classes" with a mixture of humanitarianism and fear. Philanthropies were organized, but at the same time some business leaders wanted to tear down the slums and replace them with parks. Failing that, businessmen supported increased police protection. In the end, well-to-do whites simply fled, out along new streetcar lines to the new suburban enclaves.

As well-to-do whites fled, political control of Nashville fell to city bosses, the most prominent of whom was Hilary Howse. In return for support, Howse and other bosses provided the black and white poor with more than they had ever received from the business elite. Attempts by the elite to diminish the bosses' power by disfranchising blacks (and poor whites) and catching Howse in a financial scandal in 1915 (in which the city was forced briefly into receivership) were only temporarily effective.

Doyle is at his best in linking Nashville's history to national and sectional trends. Especially effective is his mixed evaluation of the city's New South business elite and his treatment of the many reform movements that affected the city. His research is thorough and his writing is clear and strong.

"New social" or "new urban" historians may be disappointed by this study, for in spite of some effort by Doyle to address their concerns he essentially approaches Nashville's history from the top down. Somewhat more troublesome is the treatment of conflicts between old and new elites that took place in the 1880s and again after World War I. Specific points of conflict are never delineated clearly, and how these groups interacted remains cloudy.

In sum, however, Doyle has written an important volume in the growing number of studies of New South cities. The history of this proud and segmented city is a complex one, and for the most part

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Doyle has accomplished his task with thoroughness, intelligence, and grace.

WILLIAM BRUCE WHEELER
University of Tennessee

VALERIE STEELE. *Fashion and Eroticism: Ideals of Feminine Beauty from the Victorian Era to the Jazz Age*. New York: Oxford University Press. 1985. Pp. xviii, 327. $24.95.

Long confined to the periphery of scholarly concern, the subjects of fashion in clothing and physical appearance are moving to the center of interest. This new emphasis has partly to do with the rise of social history and popular culture. Additionally important have been structuralism and semiotics, with their focus on signs and symbols. Aside from language, the body and its ornamentation may afford the most basic insights into a culture's core. Thus, within the last five years have appeared, among other books, Anne Hollander's *Seeing through Clothes*, Alison Lurie's *The Language of Clothes*, David Kunzle's *Fashion and Fetishism*, Susan Brownmiller's *Femininity*, Robin Lakoff and Raquel Scherr's *Face Value*, and my own *American Beauty*.

The quest of these writers to understand culture through dress and physical appearance is both bold and perilous. Social evaluations of beauty and a society's styles of dress are easy enough to determine, but exactly what these signified in broader terms is elusive. Whatever theoretical perspective is used must be based on both immersion in sources and sensitivity to their total message. Failure to observe these guidelines is a central problem with Valerie Steele's *Fashion and Eroticism*. The message of this book is that general historical development and social change play little role in determining fashions in dress and physical appearance. Rather, eroticism is the driving force, the upper classes and fashion designers are always the controlling cadre, and changes in visual perception over time are the real reason for changes in cultural ideals.

Abstracting these ideas from a number of recent theorists, Steele has written a book that is partial and misleading. On most matters she and I disagree. There is no question that eroticism was involved in nineteenth-century fashion, but so was prudery, and the tension between these two forces lies at the heart of Victorianism. Changes in visual perception were undoubtedly central to changes in definitions of beauty, but so were the social events that lay behind those visual shifts. Designers and the upper classes played a central role in defining and disseminating fashions in dress and in beauty, but they operated in the context of a broader continuum of classes and subcultures from which they were not isolated. Eroticism as form and force, for example, had its most explicit nineteenth-century expression in a subculture of sensuality associated with the theater, a nineteenth-century institution that, as both symbol and actuality, crossed class lines in a variety of ways to permeate the consciousness of the age.

Steele and I further disagree on the relationship between feminism and fashion. The former, she argues, has never had an impact on the latter. The error she makes is to construe fashion as unidimensional. Indeed, even elite fashion (which Steele confuses with fashion more generally defined) was never impermeable to broader cultural forces. Among the most important of these forces during the last two centuries has been feminism, both as a focused ideology and as a broad social construct. Furthermore, Steele incorrectly thinks that all feminists decry fashion, which she views as liberating rather than constricting in the lives of women. Such a perspective involves her in the highly problematic arguments that tight lacing was not detrimental to women's health and that twentieth-century athleticism for women is just as much "culture bound" as nineteenth-century invalidism.

I do not mean to suggest that Steele has not done research in historical sources. But there is much more in those sources than what she has presented. Moreover, her stated methodology of looking at the extant clothing in costume collections and then attempting to construct an interpretive framework is perilous. The artifact is not the ultimate source of truth about its own meaning. And the use of theory must be combined with the fullest sensitivity to retrieving the densest possible picture of the past.

LOIS W. BANNER
University of Southern California

ROBERT W. RYDELL. *All the World's a Fair: Visions of Empire at American International Expositions, 1876–1916*. Chicago: University of Chicago Press. 1984. Pp. x, 328. $27.50.

Robert W. Rydell's *All the World's a Fair* is an original, lively, unflattering look at America's international fairs from Philadelphia's Centennial Exposition (1876) through San Francisco's Panama-Pacific Exposition (1915) and San Diego's Panama-California Exposition (1915–16). Rydell offers no broad retelling of this story. Instead, against the backdrop of social disorder in capitalist America, he sees the fairs as "triumphs of hegemony" (p. 2) through which the country's elites disseminated their values "in an updated synthesis of progress and white supremacy that suffused the blueprints of future perfection offered by the fairs" (p. 4). No loose reflection of contemporary attitudes, these fairs constituted a precisely crafted and effective response to lower-class discontent; the nationalism and racism they

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exhibited to fairgoers "became crucial parts of the legitimizing ideology offered to a nation torn by class conflict" (p. 236).

This aggressively formulated thesis brings a healthy, corrosive analysis to a subject too often treated from the standpoint of the elites themselves. But how is the argument meant? To assert that the doctrines of progress and racism broadcast at the fairs were functional for American capitalism is one thing; to suggest that the businessmen who originated and supported the fairs understood this and intended it is another. In Portland in 1905, for example, although the fair's backers fretted over union objections to open-shop policy and threats of a statewide referendum on the fair, they neither spoke nor acted as if they suffered acute anxiety. Their expressed and conscious intention in mounting the fair was to attract population to the region and to demonstrate their own managerial prowess. Although they sought a large attendance—midway exhibits of exotic peoples were seen from this standpoint—their efforts to attract workingmen to the fair were never given the considered attention that Rydell's account implies. A workingman's day with reduced admission was designated as at other fairs, but no special effort was made to counter the drawing power of a popular Portland amusement park that opened the same summer as the fair.

The question of conscious intent versus unconscious function is also ambiguously treated in Rydell's discussion of racism. He is especially critical of some of America's premier anthropologists—"high priests" of industrial capitalism (p. 235)—who, particularly through the Smithsonian's Bureau of Ethnology, offered crucial guidance in organizing the fairs' displays of different peoples or "races." That anthropology gave scientific sanction to racial ideas is clear; a prime accomplishment of Rydell's book is his demonstration of just how this was done in the more formal exhibits and on the midways.

Rydell's characterization of anthropology, however, understates both the widespread intellectual respectability and the conceptual looseness that the term "race" had at the time. It seemed, and was, a significant intellectual task for anthropologists to clarify just what "race" meant; conversely, there were profound difficulties in discrediting the concept, as the career of Franz Boas shows. A deeper exploration of the motives of anthropologists, beyond the desire to popularize their discipline, cannot be avoided if their role at the fairs is to receive a proper estimate.

JOHN TOMSICH
Reed College

WILLIAM T. HAGAN. *The Indian Rights Association: The Herbert Welsh Years, 1882–1904*. Tucson: University of Arizona Press. 1985. Pp. xi, 301. $21.95.

During the final two decades of the nineteenth century, the Indian Rights Association emerged as the leading organization in the United States devoted to the welfare of American Indians. Founded by the wealthy Philadelphian Herbert Welsh, the association quickly gained wide influence in Indian affairs. In keeping with the conventional wisdom of the era, Welsh and his associates believed that assimilation was the means to present and future prosperity for Indian individuals and communities; thus, holding private property, learning the English language, and converting to Christianity would contribute to Indian well-being. At the same time that members of the association embraced certain tenets of American life, they recognized that in a rapidly changing society injustice could prevail. Indian lands and resources, for example, frequently were sought by outsiders determined to gain control through a variety of means. At the behest of Indian peoples and through their own initiative, the Indian Rights Association became involved in many arenas of conflict. Always in earnest if not always successful, the organization labored mightily for what it viewed as the best interests of the Indians.

The importance of the Indian Rights Association has long been recognized. In William T. Hagan's long-awaited study, we now have a first-rate narrative of the association's first years. In the author's words, this book traces "the circumstances of the association's origin, its mode of operation, and its impact on the implementation of government Indian policy and the Indians themselves" (pp. ix–x). Based on the massive Indian Rights Association papers as well as careful research in the National Archives and other appropriate sources, the book provides a clear and insightful perspective on the activities of Welsh, Charles Painter, Matthew Sniffen, and Samuel Brosius. It delineates the attitudes and actions of influential policy makers such as Henry L. Dawes, Francis Leupp, Thomas Jefferson Morgan, Richard H. Pratt, and Theodore Roosevelt. The book also describes the critical challenges confronting Indians at the close of the frontier. Finally, it tells us much about the strengths and weaknesses of the policy-making process as carried out in the United States between 1882 and 1904.

Hagan's work has always been characterized by thorough research, dispassionate analysis, and an appreciation for the complex nature of relations between Indians and whites. *The Indian Rights Association* is no exception. Its balanced presentation allows us to understand the significance of the association and the qualities of the men who spearheaded it. While acknowledging the good intentions and the widespread efforts of the association and its leaders, Hagan also brings forth their failures and their shortcomings. This book is an important contribution to our understanding of Indian policy and

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Indian life of a century ago. It would also be useful reading for policy makers of this generation who continue to grapple, under somewhat different circumstances, with issues relating to Indian lands and resources and the place of American Indians in American society.

PETER IVERSON
University of Wyoming

MARTHA C. KNACK and OMER C. STEWART. *As Long as the River Shall Run: An Ethnohistory of Pyramid Lake Indian Reservation*. Berkeley and Los Angeles: University of California Press. 1984. Pp. xvii, 433. $28.50.

In 1975 Omer C. Stewart, an anthropologist and an experienced witness before the Indian Claims Commission during the 1960s, was retained by the Department of Justice to prepare an ethnohistory of the Pyramid Lake Paiutes in support of their claim to the waters of the Truckee River. Martha C. Knack served as Stewart's research assistant, and it is she who revised the legal report to produce this book.

The discipline of ethnohistory was born in the reports that anthropologists and historians wrote for the Indian Claims Commission in the 1950s and 1960s. This book is a particularly fine example of the melding of anthropology and history to produce a portrait of a people from prehistoric to modern times. It is probably a forerunner of other books that will result from current efforts to assert Indian rights to western water under the so-called *Winters* doctrine. Historians will be disappointed that the publisher did not insist on a more standardized and informative method of citation of archival sources, but they will be enlightened by the early chapters on Paiute culture, the description of the subsistence economy that Paiutes perfected before the arrival of the whites, and the historical reconstruction of interaction between Paiutes and whites from the 1850s to the present. The problem of citation is one that should be explored at the annual meeting of one of the professional historical or anthropological associations.

Although this book has many virtues, its major contribution is to our understanding of how primitive subsistence cultures were victimized by the introduction of the marketplace economy. The Paiute Indians of Pyramid Lake are a people who adapted to a harsh environment by hunting, fishing, and gathering. In a region where the food supply was often undependable, the one item that was dependable and seemingly unlimited was fish. The survival of the Paiutes virtually depended on the survival of the Lahonton trout and the *cuiui*, a species that inhabits only Pyramid Lake. From Pyramid Lake these major staples of the Paiute diet migrated up the Truckee River each year to spawn; the fry then returned to the lake to develop.

With the coming of the miners in 1859, the fragile subsistence economy began to disintegrate. The miners denuded the sparsely timbered areas for shoring timbers and fuel, destroying the piñon trees whose seeds were one source of Paiute food. Suppliers of beef appropriated the grasslands whose seeds, berries, and small game formed another part of the food chain. Both the miners and the cattlemen seized water holes and diverted streams, and squatters later challenged Paiute ownership of irrigible lands. Commercial fishermen encroached on the lake itself; after they were repulsed, the state of Nevada enacted legislation banning the commercial sale of trout completely. By this time, pollution from lumber mills and irrigation demands on the Truckee River from the Newlands Reclamation Project had already blocked the annual migration of spawning fish from Pyramid Lake. The Paiutes' last and most important food source was gradually being depleted, and, worse, the level of Pyramid Lake was declining precipitously, perhaps irreversibly. Reduced to pauperism and scavenging in nearby towns, the Pyramid Lake Paiutes were derided by whites as lazy, ignorant, and irresponsible.

The failure of the federal government to protect the resources of the Paiutes is adequately documented in this study. It is a model deserving of imitation by historians who may be called on to participate in similar instances of litigation.

LAWRENCE C. KELLY
North Texas State University

PETER LEVINE. *A. G. Spalding and the Rise of Baseball: The Promise of American Sport*. New York: Oxford University Press. 1985. Pp. xv, 184. $16.95.

A good case can be made that Albert Goodwill Spalding was the most significant single figure in late nineteenth-century American sports. Star pitcher for the powerful Boston Red Stockings in the old National Association and then player, captain, and manager for the Chicago White Stockings in the newly formed National League, Spalding was already nationally known as a professional baseball player by the time he hung up his uniform in 1877 at the age of only twenty-seven. Spalding then concentrated his energies on building the Chicago team, of which he eventually became principal owner, into the most successful and most profitable in baseball and building his little sporting-goods business into the world's foremost manufacturer and marketer of athletic equipment. By the end of the century he was rich and famous, and the most influential person in baseball. From 1900 to his

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death in 1915, Spalding lived in nominal retirement near San Diego with his second wife. He supported (but never converted to) the theosophy movement, made an unsuccessful bid for a U.S. Senate seat, finished his weighty autobiography, and continued in various ways to promote baseball in particular and athletics in general as the means to a wholesome, disciplined, and patriotic citizenry.

Peter Levine's rather ponderously named book is the first close study of Spalding's career since Arthur Bartlett wrote *Baseball and Mr. Spalding* thirty-four years ago. Unlike Bartlett, Levine seeks to interpret Spalding as a man of his time: a self-made millionaire convinced that hard work and determination would pay off for everybody; a professional, an expert, and a corporate builder working for order and stability in a growing new industry; a sportsman imbued with the belief that life's lessons could be learned most directly on the playing field. Yet, while Levine does a good job of putting the man and his opinions in context, Spalding appears here more as a type than as a distinct personality. The reader learns little about the nonpublic Spalding—about his relationships with his parents, brothers, first wife, children, and baseball intimates. Suddenly, near the end of his book, Levine tells us that, two years after Josie Spalding died, A. G. married Elizabeth Mayer Churchill, a woman with whom he had secretly carried on an affair "for years" and fathered a son. Surely that episode in the life of a man who projected such an image of rectitude is worth more than mention.

Levine's research was thorough. Besides combing the *Sporting News*, *Sporting Life*, the New York *Clipper*, Spalding's annual baseball guides, and various daily newspapers, Levine used the substantial baseball-related materials at the Chicago Historical Society. As a study of Spalding's role in the emergence of professional sports, Levine's book is a valuable addition to a fast-growing area of scholarship. No more than Bartlett, however, has he given us a full biography. That is largely a consequence of his book's brevity, for Spalding's life deserves more than 147 pages of text.

CHARLES C. ALEXANDER
Ohio University

DONALD HARMAN AKENSON. *Being Had: Historians, Evidence, and the Irish in North America*. Port Credit, Ont.: P. D. Meany. 1985. Pp. 243. $45.00.

Donald Harman Akenson continues with this book his spirited inquisition of his colleagues who share an interest in Irish and Irish-American history. The book is an extension of the historiographical sallies and analytical challenges set forth in an earlier work, *The Irish in Ontario* (1984). In the present volume he asserts that "even our profession's worst failures can be put to positive use as lessons that will help us do a better job" (p. 137), and he then proceeds to expose and dissect failures in source analysis, abuse of data, and erroneous generalizations in Irish-American studies. In chapters that are both acute and professionally entertaining, he catalogues the gullibility of scholars, the follies of the committees of the American Council of Learned Societies, and defects of U.S. census materials on immigration, as well as the emphasis of historians in the United States on the urban experience of the Irish-Americans. As the bulwark for his critique, the author extols the use of Canadian data, such as those he used in his study of Irish settlement and influence in Ontario, and caps his argument with the judgment that "historians who have dealt with the Irish in North America have been lax in their standards of evidence and have framed their basic story of the Irish in the United States and in central Canada on a remarkable paucity of solid evidence and upon a gross misreading of what evidence they actually have found" (p. 189).

Aside from chiding his colleagues, Akenson makes several contentions that are important for scholars of immigration, ethnic studies, and Irish historical inquiry. He believes that the impact of the Irish on the formation of Canadian society, or at least of English-speaking Canadian society, was greater than their impact on the United States—a broad and interesting thesis for debate. He protests the truncation of immigrant studies of the Irish that take up the study of the group only at the time of the famine emigration of the 1840s, an important correction to the foreshortened focus of work in this field. In a concise commentary on the assumptions that have guided Irish-American studies, Akenson dissents from the reasoning that has described the group as habitually and decisively urban in its settlement patterns. This latter view requires that a distinction be made between settlement patterns and eventual social and political influence, a distinction to which the author adverts too narrowly.

Akenson's ten previous books on Irish social history certainly confer on his work an authoritative quality. The present book is an engaging tour de force written with humor and critical inventiveness.

His presentation solicits a much-needed review of just how American scholarship has dealt with the record of one of the largest groups whose fortunes have shaped development in the United States. His arguments about the use of evidence and the premises for research may be uneven, but they are penetrating. The book, using the study of Irish-Americans as case material, highlights the role of historiographic fundamentals such as the scope of inquiry, the assumptions preceding investigation, and the logic of exposition, elements all too fre-

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quently obscured in our usual fixation with fragmentary and specialized research.

DENNIS CLARK
Samuel S. Fels Fund
Philadelphia, Pennsylvania

DENNIS CLARK. *The Irish Relations: Trials of an Immigrant Tradition.* Rutherford, N.J.: Fairleigh Dickinson University Press. 1982. Pp. 255. $24.50.

The title of this book is inexact. This is a history of the working-class Irish of Philadelphia. First to be examined are the early social and economic problems encountered by the Irish during their adjustment to their new urban world. Thereafter, Irish relations with blacks, Jews, and rich whites are explored. The political and cultural history of the Philadelphia Irish brings the story down to the present time. If the study of America's first major immigrant group within such a historical setting may be characterized as "local history," this volume has succeeded at just that.

Dennis Clark's academic field is American urban history, and he is a major contributor to the growing historiography on the American Irish. This is his third such book. His others have treated the broader aspects of Philadelphia life and history as well as national urban problems.

The Irish Relations is characterized by breadth of treatment rather than by depth of analysis. It provides a highly readable account of a subclass within an ethnic group, one that presumably left behind few records. Clark's energetic research disproves that presumption. The result is a comprehensive, narrative history guided largely by a thorough understanding of the broader historiography of the field. Nowhere, however, does the Philadelphia story challenge major concepts that sustain the field of immigration history. The book extends knowledge; it does not prompt rethinking.

The chapter on indentured children documents that the practice existed as late as 1866. Before the Industrial Revolution changed the conditions of child servitude, even antislavery Quakers dealt in Irish child contracts. The hazards encountered by Irish labor at the Du Pont gunpowder works seem more than adequate to illustrate Clark's assertion that the Irish "were the group most exploited by industrialization when the process was in its crudest and cruelest stage" (p. 46).

This work did not benefit from *Erin's Daughters in America* by Hasia R. Diner. The chapter "Women of the House" is relatively undeveloped. Clark's independent conclusion is nonetheless similar to Diner's: given a starting point of rural Irish poverty, for a woman to preside over a clean American household represented an accomplishment. The two authors disagree over Irish representation in prostitution. Clark reiterates his previously published acceptance of the conventional wisdom that Philadelphia prostitutes were mostly Irish, whereas Diner cites statistics to demonstrate quite the reverse.

On stronger ground Clark details new minibiographies of Irish nationalists who made Philadelphia such a prominent center for organized benevolence and violence. Among these, John J. Reilly, a transitional figure, helped refurbish Ireland's reputation, which had deteriorated in America as a result of Irish neutrality during World War II. His attempts, during the 1940s and 1950s, to prompt the involvement of the U.S. government in the resolution of the then-dormant problem of Northern Ireland met, unfortunately, with only a yawn.

This is a worthy study of the working-class Irish of Philadelphia, how they lived among themselves, and how they got along (or did not) with the significant other groups who lived around them.

JAMES P. WALSH
San Jose State University

JON GJERDE. *From Peasants to Farmers: The Migration from Balestrand, Norway, to the Upper Middle West.* (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1985. Pp. xiv, 319. $32.50.

This is a study of transatlantic migration, settlement in the Upper Midwest, and adaptation to the American environment by people from Balestrand in Sogn, a small fjord community on the west coast of Norway. Jon Gjerde contributes an American perspective to the growing scholarly literature on the nineteenth-century overseas movement from individual communities throughout Norway. In 1972, Andres A. Svalestuen provided a useful model in his examination of the exodus from the mountain community of Tinn in Telemark from 1837 until 1907, using methodology that relied heavily on local demographic, economic, and social development. Norwegian community studies have generally abandoned the immigrants at water's edge; thus, the story of settlement in and adaptation to the new country has been largely ignored. These investigations, however, revealed the diverse characteristics of geographical mobility and the complexity and variety of migrational forces.

Gjerde is indebted to these local studies. He adopts the analytical techniques and quantitative source materials of the Norwegian works, but he manages to move beyond the parochial level of some of them and to place a local migrational phenomenon in the universal context of transformation and cultural change. Responding to Frank Thistlethwaite's influential prescription of 1960 "to

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treat the process of migration as a complete sequence of experiences," Gjerde links the Old World background of immigrants to their adaptation in the place of arrival.

The movement overseas from Balestrand, commencing in the mid-1840s, was a rural-to-rural migration, with a sizeable portion of the migrants settling in contiguous areas. The author argues convincingly against the widely accepted explanation that heavy, early emigration from Norwegian fjord and highland districts was caused largely by rapid population growth that outstripped the limited resources in that tradition-bound agricultural society. In discussing the situation in Balestrand, Gjerde instead emphasizes the economic and structural changes that were creating an increasingly landless society, making households based on land ownership impossible for most residents. The opportunity to acquire land in the United States became a convenient solution for land-hungry peasants; in America a traditional way of life could be preserved as Norwegian peasants became American farmers.

Although resources in Balestrand were adequate and, as Gjerde maintains, circumstances even improved for all social classes during the emigration era, the expanding population required new household strategies and social patterns of behavior to meet the increased demands on local resources. Economic practices were altered to produce more food; social behavior responded in the areas of courtship, marriage, and fertility. Pietistic awakenings condemned night courting and prenuptial births not followed by marriage, thus reinforcing the community's moral teachings and encouraging fertility control.

Opportunities in America, where land was cheap and labor expensive, intervened to alter immigrant practices. Reacting to environmental influences, the immigrants behaved differently from their kin in Balestrand even while retaining cultural expressions from the home community. The family farm in America, lacking the retinue of servants and cotters common in Norway, encouraged greater marital fertility to produce needed labor; the sexual division of labor changed as women moved further into domestic roles; Norwegian courting patters were modified, in part because of pietistic attitudes rooted in the religious awakenings in Norway. Greater wealth allowed the immigrants to imitate urban patterns of behavior in housing, dress, household amenities (such as the purchase of pianos), and leisure activities. But the bourgeois life style was colored both by a unique Balestrand cultural background and by the dominant position of the immigrant Lutheran church.

Gjerde delivers a well-conceived, clearly formulated, and thoroughly researched scholarly investigation of the migration of a particular group, from the Norwegian homeland to a new life in America. This book is an important contribution to our knowledge and understanding of the transatlantic cultural impulses produced by the mass migration of the last century. In addition, the study contains a superbly interpreted and insightful survey of nineteenth-century Norwegian peasant society.

ODD S. LOVOLL
St. Olaf College

DONALD E. PIENKOS. *PNA: A Centennial History of the Polish National Alliance of the United States of North America.* (East European Monographs, number 168.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. xxi, 485. $37.50.

The Polish National Alliance (PNA), founded in 1880, is the largest ethnically based fraternal society in the United States, with nearly three hundred thousand members and assets totaling $176 million. It has also been the most important source of secular leadership in the Polish-American immigrant community. The PNA has commissioned five official institutional histories, but Donald E. Pienkos's centennial history is the first in the English language.

In *Historja polska w Ameryce* (1905) Wenceslaus Kruszka, the first historian of Polish America, penned this much-quoted phrase: "For a time neither Catholics nor Poles were to be found in America but only 'Unionists' [adherents of the Polish Roman Caholic Union (PRCU)] or 'Alliancers' [PNA adherents]. He who did not belong to the Alliance was regarded as no Pole while whoever was not of the PRCU could not be accepted by the Unionists as a Catholic" (vol. 4, p. 32). Kruszka overstated his case, but the statement does point up the pervasive influence of the two leading fraternal organizations on the lives of Polish-Americans and the bitter ideological conflict between the PRCU and the PNA. Clerical leadership steered the PRCU into a conservative stance on the liberation of Poland, whereas the lay, nationalist leaders of the PNA labored long and hard for the restoration of Polish independence. In its internal policies, the PRCU remained steadfastly Catholic, but, in accord with its secular and nationalist character, the PNA adopted no religious test for membership.

Pienkos's book examines the PNA's history in comprehensive fashion, and the author continually relates the fraternal organization's institutional history to larger events in Poland and America. In the chapter on World War I and Poland's rebirth, he does a particularly skillful job of drawing together the many personalities, organizations, and events that led to Polish independence. The chapter on

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Charles Rozmarek's stormy presidency of the PNA (1939–69) is simply fascinating. Rozmarek built a political machine in the Chicago-based fraternal union, which will remind readers of Chicago politics under the late Richard J. Daley.

Pienkos made a few questionable editorial decisions. He divided the text into two parts, a chronological history of the organization and a series of topical chapters discussing such matters as the ethnic press, educational activities, and the role of women. This leads to an excessive amount of repetition, for he provides little new material in the topical chapters that is not mentioned at least once in the chronological section. Pienkos has also picked up on the unfortunate usage of the word "Polonian" as either an adjective or a noun. "Polonia" (*Polonja Amerykańska*) has long functioned as a useful collective noun denoting all Polish ethnic communities in America, but the people who live in those neighborhoods should be called "Poles" or "Polish-Americans." ("Polonian" sounds like something out of *Star Trek*: "Look, Captain Kirk! The *Enterprise* is being attacked by Polonians!") I devoutly wish that all scholars writing about Polish-Americans would ban this word forever.

Such quibbles aside, Pienkos has written an important and useful study of a major ethnic institution. Such a publication in English was long overdue. Perhaps the PRCU will now commission a competitive volume.

EDWARD R. KANTOWICZ
Chicago, Illinois

MARC LEE RAPHAEL. *Profiles in American Judaism: The Reform, Conservative, Orthodox, and Reconstructionist Traditions in Historical Perspective*. San Francisco: Harper and Row or Fitzhenry and Whiteside, Toronto. 1984. Pp. x, 238. $19.95.

The theme of this analytical yet readable work is that both Orthodox and Conservative Judaism in America eventually developed their own responses to the central influence of Reform Judaism. This book is a bold departure from the accepted thesis that orthodoxy originated among the first Jewish families in America.

Marc Lee Raphael is not inclined to dwell on the obvious: that traditional ("historical") Judaism, far older than America, appeared before the other persuasions. But in his emphasis on "institutionalization" (including the formation of a rabbinic union) orthodoxy is viewed as maturing only in the twentieth century. Thus, in 1901 the first president of the Union of Orthodox Jewish Congregations of America attributed the organization's emergence to a "protest against 'declarations of reform rabbis not in accord with the teachings of our Torah'" (p. 136). Such disputes did not preclude, of course, a shared concern with social justice among divergent Jewish traditions.

The author uses a social science approach succinctly and with relative objectivity, including also an analysis of respective leaders and ideas. The documentation is impeccable and his text is a worthy handbook for scholars of religions. Still, one wonders whether the beliefs and practices of countless individuals, along with their response to competing sectarian requirements, can truly be assessed. We have records for influential and worldly American Jews, but how representative are their perspectives in the realm of private devotion?

Reform Judaism, rooted in the European Enlightenment, seemed ideally suited to nineteenth-century American liberalism. It embraced individual freedom rather than communal authority or divine ordination as well as respect for reason rather than for the supernatural. "Whatever philosophy and science have overcome, is dead," proclaimed Rabbi Isaac Mayer Wise (p. 22), a view shared by his colleagues from a German cultural background. Max Lilienthal, who helped introduce Wise's *Minhag America* (American Ritual) revised prayer book to his Cincinnati synagogue, proclaimed in numerous churches a "doctrine of universal unsectarian love" (p. 13).

Optimism, universalism, and modernity were hallmarks of Reform. Liberating Jews from the constraints of tradition also made them more acceptable to the majority and appear more deserving of socioeconomic advancement. If emancipation proved a vain hope in Europe, it offered far more in a free America. Reform was, in part, a kind of pragmatic accommodation, having social goals rather than revelation as its principal focus. No wonder, as Raphael suggests, a "multiplicity of Jewish theologies constitutes Reform Jewish thought"; in Alvin Reines's words, "all opinions" are "equally valid" (p. 56). In America the new dispensation included not only the aesthetics of sacred music, decorum, and mixed seating but also the "attraction of enlightened Protestant simplicity" (p. 10).

The shibboleths of acculturation seem moot in the wake of the Holocaust. Pervasive racism in the nineteenth century hindered acculturation; enlightenment ideals were nowhere widely accepted. Yet Reform advocates embraced Americanization zealously, despising the Yiddish "jargon" and Zionism of East European immigrants, as well as their orthodoxy and "Oriental" manners. Eventually, however, the influence of East European Jews swung the pendulum toward a more traditional and ethnic orientation, especially in the face of weak temple attendance and the apparent passivity of worshipers. Still, new sources for pluralism mingled with the old: the language of prayer (English and Hebrew),

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mixed marriage, women as rabbis, and the Reconstructionists' "historical" Judaism independent of revelation.

JOSEPH BRANDES
William Paterson College of New Jersey

JAMES D. BRATT. *Dutch Calvinism in Modern America: A History of a Conservative Subculture*. Grand Rapids, Mich.: William B. Eerdmans. 1984. Pp. xiv, 329. $13.95.

The key to understanding this book is to know that it is about Dutch Calvinism, not Dutch Calvinists. Using Dutch leaders' dedication to pure Calvinist theology as the centerpiece, James D. Bratt offers to students of religion in America a carefully researched analysis of the acculturation of this immigrant group to American society. Were a prospective reader to see this work and expect from it an *Annales*-oriented look at the everyday lives of ordinary people, that reader would be disappointed. Recognizing this limitation, the author defines his boundaries precisely and, concentrating on the twentieth century, explores the intellectual history and the substance of the religious thought of Dutch immigrant leaders in the United States.

What stands out is the agony and divisiveness experienced by these spokesmen in America, who sought to interpret their religious experience and set the standards for theological truth through the eyes of Dutch Calvinism. How so dogmatic a thought system as Calvinism could lead to divisiveness is made understandable by Bratt's division of the Dutch family into four mentalities: Seceders and Pietists, Confessionalists, positive Calvinists, and antiheretical Calvinists. He shows with great clarity how and why these factions emerged and why, even in a new country not especially receptive to the theological disputation, the arguments among the factions lasted as long as they did.

Two features stand out from this extended dialogue: the success with which the participants were able to end the potentially meliorating influences of twentieth-century American theologians, who were light years removed from the Calvinists, and the irrelevance of these disputes to the needs of the Dutch Americans. Bratt points out in his discussion of the 1920s and 1930s that the Calvinist emphasis on personal piety "failed before the mounting horrors of the time" (p. 141). Writing, as the leaders and editors in the seminaries and colleges did, about family issues, the need for chastity, and avoiding movies, dancing, and alcohol "in the face of Depression, war, and concentration camps was to forfeit too valuable a part of the Calvinist tradition, not to mention of common humanity" (p. 141). The author argues persuasively that "other visions" for life in America among the Dutch were needed. As long as the intellectual elites focused on internal doctrinal disputes, such as holding heresy trials for dissident seminary professors, constructive responses from Dutch Calvinists were not to be forthcoming.

Lest one think this narrative centers only on theology, the bonus of a chapter on "four renegade novelists," all of Dutch origins, is a delightful surprise and impressive innovation. Using their works as examples of the "rebels" among those who rejected the doctrinal wars of the academies, Bratt outlines the major themes in the novels of Arnold Mulder, David Cornel De Jong, Frederick Manfred, and Peter De Vries. The novelists are interpreted as important contributors to a different, more humanistic expression of the Dutch voice in American life. So well done is this chapter that one looks for parallel contributions by Dutch Americans in other areas such as the fine arts.

A concluding chapter brings this story through the late 1970s, emphasizing both the continuity and the new outreach of the movement. No longer is the center of power in Grand Rapids, nor are the demands on the intellectual elite purely theological. A new generation of well-educated spokesmen attempts through vehicles such as *The Reformed Journal* to explore traditional Calvinist dogma in the arts and sciences. Bratt clearly endorses their efforts.

This book is for specialists and assumes considerable prior knowledge about general immigrant history. It unabashedly concentrates on theological minutiae and makes a convincing argument that this group is truly a "conservative subculture."

ERLING JORSTAD
St. Olaf College

DAVID NASAW. *Children of the City: At Work and at Play*. Garden City, N.Y.: Anchor/Doubleday. 1985. Pp. 244. $18.95.

David Nasaw focuses on children who shined shoes and sold newspapers as well as a variety of other goods—gum, candy, flowers—to customers on city streets in the first two decades of the twentieth century. These children controlled a portion of the streets, where they worked, played, made money (which they spent on new cheap forms of mass amusement), and attracted the attention of zealous reformers. Most of the street traders were boys, although girls were always present. Typically, however, girls worked at home doing piecework or providing child care. According to Nasaw, the reformers were no match for the sharp wits and good sense of the street children. These shrewd young businesspeople knew how to find a good location, make a quick sale, get a good tip, and avoid the cops.

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The most dramatic moment in the history of the street children was the great "newsie" strike of 1899, when a children's union forced Joseph Pulitzer and William Randolph Hearst to alter the distribution practices of their newspapers. The success of the strike pointed to the importance of cooperation and democracy among the young street vendors, but this golden age of street trading came to an end about 1920 as newspapers began emphasizing home delivery.

The greatest virtue of Nasaw's book is that it shows us children as historical actors. Much of the author's material comes from the numerous reports and analyses written by Progressive reformers and from the thousands of photographs taken by reform-minded photographers such as Lewis Hine. By supplementing these sources with autobiographies, including those of well-known entertainers, Nasaw constructs a lively and fascinating account of what children of the streets thought and did.

The title may lead some readers to believe that they will find a comprehensive treatment of children in an urban setting in the early twentieth century, but *Children of the City* emphasizes children on the principal business streets of the city and says relatively little about them in other contexts. One chapter examines conflicts between children and parents over spending money, and another explains how girls fit into city life at the turn of the century. Because of Nasaw's romantic style, the book reads like a melodrama in which wily kids continuously outwit Keystone Cop–like reformers. Nasaw combines nostalgia for the golden age of street trading with a tendency to explain events of the period in a simplistic way.

These concerns are minor in comparison to the strengths of the work. *Children of the City* is important because it adds to our knowledge of what cities were like at the turn of the century and because it shows that historians can treat children as historical actors whose actions and thoughts made a difference. This is a delightful book to read; both students and scholars will enjoy it and learn from it. The scholarship and documentation are superb, and no historian of American society, American cities, American families, or American childhood can afford to miss it.

JOSEPH M. HAWES
Memphis State University

STEVEN L. PIOTT. *The Anti-Monopoly Persuasion: Popular Resistance to the Rise of Big Business in the Midwest.* (Contributions in Economics and Economic History, number 60.) Westport, Conn.: Greenwood. 1985. Pp. x, 194. $35.00.

Steven L. Piott contends that big business and democracy are, in some nebulous sense, incompatible. He uses the popular press, primarily newspapers, to establish painstakingly that numbers of people at the turn of the ninteenth century shared his concern. Piott's narration of events is good, but he vitiates his argument by using his sources in a disappointingly unanalytical fashion. Probing questions about the validity of the emotionally charged rhetoric—such as how many articles were written on the subject, how often they appeared, and how representative they were—are left unasked. Furthermore, the author rarely evaluates his evidence by asking, how true is it?

The author emphasizes symbolic politics and neglects industrial organization economics to the detriment of a complete understanding of why competition among the few came to characterize the meatpacking industry, among others. He therefore blurs the distinction between the predatory practices of individual firms and the evolution of managerial enterprise, both of which are tarred with the same brush in his eyes and in the eyes of a group that he treats uncritically—those who resisted the rise of big business. Presenting the critics of big business as people who served the public good, he fails to recognize that they were also self-serving. The old independent middle class employed the political process to erect barriers to the interstate movement of low-cost goods produced by large-scale enterprises and instituted resale price maintenance (fair trade) to protect their smaller businesses. Piott does not explain why he gives credence to the definition of a moral economy made by the critics of big business, nor does he identify at what specific points he regards their ideas as archaic or otherwise flawed.

The businessman was the revolutionary in the Industrial Revolution. That some of those who were adversely affected by the growth of managerial enterprise, and whose judgment therefore may have been warped by self-interest, imagined conspiracies or demons to be the prime movers of industrialization in no way validates their perceptions. The Luddite view of economic change is not necessarily moral; it may simply be wrong, and Piott does not tell the reader why these petit-bourgeois contentions were right. Also, by presenting their belief structure without according due attention to their economic behavior, he ignores the possibility of inconsistency. Those who so vigorously and obsessively attacked managerial enterprise often were ready and willing to be its suppliers and customers. Ultimately, the conflict between efficiency and democracy is an inherently unresolved dilemma. Finally, although the author skillfully marshals his material, he never successfully justifies the conclusion stated in the

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epilogue precisely because his evidence is selective and tendentious.

SAUL ENGELBOURG
Boston University

JOHN HERBERT ROPER. *U. B. Phillips: A Southern Mind.* Macon, Ga.: Mercer University Press. 1984. Pp. vi, 198.

Historians seldom merit biographies. From time to time, however, an individual's writings tower above the historiographical landscape and cast shadows other historians cannot avoid. In such cases, we want to understand the origins of the arresting perspective and the evolution of the pivotal ideas. That requires understanding a mind, a life. Ulrich Bonnell Phillips asked what made the South southern, and for half a century his answer prevailed. In this brief intellectual biography, John Herbert Roper asks what made Phillips answer the question as he did. Roper concludes that, although Phillips wrote from more than personal experience, each of his major studies was an "analysis of self" and that Phillips's individual history was "largely in tune with the collective self of the region" (p. 130).

Born in LaGrange, Georgia, in the year in which the last federal troops marched north to end Reconstruction, the child of an aristocratic mother and a father from plain folk, Phillips felt rooted in the region's antebellum past. He grew up self-consciously and defensively southern, even to the point of changing his name from Ulysses to Ulrich to avoid the nasty association with Ulysses S. Grant. He attended the University of Georgia, where John H. T. McPherson, a former student of Herbert Baxter Adams, introduced Phillips to scientism, with its devotion to original sources and thoroughness. For graduate study, he migrated north to that bastion of scientism, Columbia University. Formally a student of William Archibald Dunning, Phillips was already a disciple of Frederick Jackson Turner and Progressivism. Scientism provided Phillips his research tools, and Progressivism—especially its emphasis on economic causation and class conflict—gave him interpretive direction. Turner also provided Phillips his dissertation topic, his first job, and an enduring model for his academic and personal life.

From the beginning, however, the power of race struggled to find its place within the framework of class analysis. The tension found expression in both Phillips's politics and writing. While insisting on preserving the South's racial hierarchy, he attempted to steer a middle course of benign racism between what he considered the Scylla of the demagogues and the Charybdis of the egalitarians. Before World War I, he supported conservative reform and was mildly critical of planter power. But after the war, Roper explains, to preserve white rule and gain "biracial salvation," Phillips defended elite domination and portrayed the Old South as ably and equitably organized by plantation paternalism. In *American Negro Slavery* (1918), more positively in *Life and Labor in the Old South* (1929), and most boldly in "The Central Theme of Southern History" (*American Historical Review* [1928]), he identified the drive for white supremacy as the dominant fact of southern life.

Like a recent biographer of William Faulkner, Roper has discovered "deep reciprocities" between the writer and his work. Phillips built much of his history of the Old South on his experience in the New South. Still, Roper sometimes strains to link incidents in Phillips's life to changes in his view of the past. One such moment—his observation of blacks at Fort Gordon in 1918—is made to explain his shift from economic to social causation, which is more weight than the incident can bear. Roper also attempts to keep score between Phillips and his critics, tallying who was right and who was wrong, an exercise that is largely beside the point in an intellectual biography. Generally, however, Roper's argument is pertinent and persuasive. His intelligent study of this "southern mind" provides us with a better handhold as we continue to wrestle with what remains one of the most powerful interpretations of southern history.

JAMES L. ROARK
Emory University

LINDA O. MCMURRY. *Recorder of the Black Experience: A Biography of Monroe Nathan Work.* Baton Rouge: Louisiana State University Press. 1985. Pp. xv, 154. $20.00.

Specialists in twentieth-century Afro-American history are rediscovering biography to weigh the conduct of important black figures and to assess the organizations that defined their careers. Louis R. Harlan's two-volume study of Booker T. Washington epitomizes this trend, and readers will think of others too numerous to mention here. The 1985 program committee of the American Historical Association scheduled a session on this very topic, and independent projects on Ralph Bunche, Mary McLeod Bethune, Carter G. Woodson, and Clarence Mitchell, among others, are underway, as are new biographies of Marcus Garvey, W. E. B. Du Bois, A. Philip Randolph, and Martin Luther King, Jr. The historiographical stakes are actually quite high. The writing of such biographies either will generate a composite, analytical awareness of black initiative and institutional life and move us significantly closer to a basic understanding of inter-

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racial dynamics or will become merely a cottage industry for the episodic rendition of events.

Against this background, Linda O. McMurry's study of Monroe Nathan Work is both welcome and disappointing. She reviews his formative years: his origins in North Carolina, a homesteader's youth in Kansas and later in Oklahoma, abbreviated service as school teacher and as an African Methodist Episcopal minister, enrollment in the Chicago Theological Seminary, and a 1903 master's degree in sociology from the University of Chicago. Thereafter, as a faculty member at Georgia State Industrial College in Savannah, he helped develop campus and community betterment projects and participated in some of Du Bois's famed Atlanta studies. From 1908 to retirement in 1938, Work labored at Tuskegee Institute, where he established the department of records and research, edited the *Negro Year Book*, published *A Bibliography of the Negro in Africa and America*, and promoted educational extension programs and National Negro Health Week. Insisting that factual evidence was a more powerful weapon than open confrontation for improving race relations, he exhibited that mixture of upward striving, race pride, and interracial cooperation characteristic of his generation.

Unfortunately, McMurry has produced a needlessly linear treatment of Work's endeavors. She cites his interests and motivations but leaves the reader to guess just how they coincided with those of his contemporaries. There is all too little discussion of internal Tuskegee affairs, of southern race relations, or of national academic and political trends. In addition, McMurry pursues with insufficient consistency and depth the nature, direction, and impact of forces that touched Work's era.

The author relies heavily on the papers of Monroe Work, Booker T. Washington, and Robert Russa Moton, on the transcript of Lewis A. Jones's 1932 interview with Work, and on a 1949 retrospective article by Jessie Parkhurst Guzman, Work's colleague and successor. These sources are not matched, however, by the rich variety of personal, organizational, and institutional materials available elsewhere for twentieth-century black scholarship. Not infrequently, then, pronouncements and speculations take the place of convincingly crafted explanations. Finally, the publisher must share some culpability: we do need a bibliography, and we should have the consistent use of page or chapter citations when secondary works appear in the footnotes.

ROBERT L. ZANGRANDO
University of Akron

THOMAS R. NEVIN. *Irving Babbitt: An Intellectual Study*. Chapel Hill: University of North Carolina Press. 1984. Pp. xi, 194. $20.00.

Irving Babbitt, the Harvard literary critic, has been a kind of missing link in American intellectual history. On first picking up Thomas R. Nevin's interesting effort at reconstruction, one can recall extensive references to, and vignettes about, Babbitt in works by men as disparate as Henry F. May, David Spitz, Jacques Barzun, Allen Guttmann, Alfred Kazin, Edmund Wilson, and Russell Kirk. All testified to his strong impact, in one way or another, on a whole generation of Harvard students, among whom Van Wyck Brooks, T. S. Eliot, Austin Warren, Harry Levin, Walter Lippmann, and David Riesman are the most notable. In such reminiscences, however, Babbitt somehow never quite came into focus. He was clearly a political conservative as well as a student of the Greek and Roman classics, French literature, and Buddhism—perhaps a "humanist" in the Renaissance sense of the word—but none of these enthusiasms added up to anything easily categorized. Nor do Babbitt's five works, when consulted by the casual inquirer, provide much illumination or any temptation to explore further.

Babbitt has now been disinterred and brushed off, painstakingly and elegantly, in Nevin's "intellectual study." His effort cannot be faulted in any major way. The book is written with style and verve. Nevin, a classicist, appears impressively familiar with the materials of Babbitt's own wide-ranging scholarly interests, although here and there slips suggest a sharing of Babbitt's own ignorance of the full texture of the American past. The documentation is admirable, and the modest brevity of the whole work is commendable. Yet somehow Babbitt's fossilized remains still do not add up to much. Two careful readings, which Nevin's work inspires but—so thoughtful is he of his readers—does not require, leave the paleontological mystery unresolved.

The trouble, one finally concludes, lies not with Nevin but with Babbitt, or perhaps with the American culture that the latter so obsessively deplored. Babbitt, one learns from Nevin's several paragraphs of biography, came from an idiosyncratic, lower-middle-class background. Not an undergraduate at Harvard until near adulthood, and only after much scrabbling and deprivation, Babbitt was heavily infused with the norms of a culture that had offered him neither rewards nor, failing that, any internalized experience that would allow him to stand apart from these norms.

Even as Babbitt became a scholar omnivorous in breadth and depth, recognition came slowly; only after decades of teaching at Harvard did tenure come in his late forties. Scholarship in the ancient classics of several cultures, plus the reading and teaching of French literature, created for Babbitt an intellectual posture he could never reconcile with his own individualistic irascibility. Paul Elmore More,

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Babbitt's closest friend, noted in the midst of a eulogy "something almost inhuman in the immobility of [Babbitt's] central ideas" (p. 128). Nevin speculates in his excellent bibliographical essay "that some personal acquaintance with Babbitt was almost a sine qua non for any judicious appreciation of him" (p. 188). One ends up wishing that Nevin's otherwise superbly conceived and executed appreciation of Babbitt had told us a bit more about the man himself. Only that, as Nevin seems tacitly to concede at several points, could restore a similitude of life to a challenging fossil.

CHARLES FORCEY
State University of New York, Binghamton

DAVID D. LEE. *Sergeant York: An American Hero.* Lexington: University Press of Kentucky. 1985. Pp. xiii, 162. $18.00.

David D. Lee announces two purposes for this book. As biographer, Lee wishes to prepare a scholarly account of Alvin York's story in order "to penetrate the myth surrounding" him and "establish the facts of his life" (p. xi). As student of the "hero-making process itself," Lee also intends to use York to illustrate "how heroes are chosen," how they "in turn manipulate the process," the "characteristics heroes exhibit, and the role institutions play in publicizing them" (p. xi).

His book's biographical dimension succeeds admirably. Although sympathetic, Lee avoids romanticizing York, quickly sketching the formative years of a rowdy though family-loving youth whose wild ways and "taste for alcohol" (p. 7) finally yielded to his sense of sin, his mother's pleas, and his own love for "Miss Gracie" Williams. Lee convincingly accounts for York's transformation from Christian pacifist to Godly crusader and judiciously describes how he captured 132 Germans on October 8, 1918. Home from France, York passed his life, except for publicity tours, in Tennessee, where he sought to bring education, religion, and prosperity to the inhabitants of Fentress County. Only the specialist will need to know more about York. The meager records of a poorly educated man living among similarly handicapped people suggest the difficulty of learning much more about York's inner life than Lee reveals.

This slim book's more ambitious assessment of the hero-making process, however, seems plausible but incomplete. York became a celebrity, Lee claims, because his personal qualities seemingly provided "balm for the anxieties that gripped American society" while it rapidly evolved from its Anglo-Saxon, rural, agricultural origins into a pluralistic, urban, industrial nation (p. x). York, the poor, pious, unschooled, pioneer rifleman who single-handedly silenced thirty-five machine guns, "represented not what Americans were but what they wanted to think they were" in a society grown both secular and materialistic (p. x). "The celebration of Alvin York," Lee writes, "was really meant . . . as a celebration of American life" (p. 68).

That is plausible, of course, but why York? A half-dozen others accomplished comparable feats. With one partial exception, however, Lee leaves their qualities unexamined and his thesis untested. By attempting to bring education and material abundance to his community, York paradoxically seemed to reject many of the qualities that allegedly made him a celebrity. The 1941 filming of York's life, moreover, created yet another image of the sergeant, whose advocacy of preparedness and aid to Britain appealed to the film makers, the administration in Washington, and Americans more anxious about war than about social change. York proved, then, a most flexible hero, molded by the media to suit a variety of contemporary purposes. In that sense York's "significance lies not in what he was but in what others wanted to make of him" (p. 115), and Lee, as student of hero making, should have told us more about the media's relations with York than proved possible in one thin volume.

JAMES L. ABRAHAMSON
U.S. Army War College

DAVID W. LEVY. *Herbert Croly of the New Republic: The Life and Thought of an American Progressive.* Princeton: Princeton University Press. 1985. Pp. 285. $32.50.

Between the publication of *The Promise of American Life* in 1909 and the beginning of World War I in 1914, Herbert Croly was recognized by his peers as the most important theoretician of American Progressivism. Subsequent generations of scholars have concurred in this evaluation. The appearance of the first full-length intellectual biography of Croly, therefore, is of great significance for scholars of the Progressive movement, of American liberalism, and of American political thought. With great diligence and skill, David W. Levy gathered materials that made it possible for him to prepare this informative, gracefully written overview of Croly's life.

His research has been most successful in recreating the world of Croly's parents, Jane Cunningham Croly and David Goodman Croly, both important journalists in New York City, and in depicting their contribution to the shaping of their son's attitudes toward history. Levy does not employ a psychohistorical approach, but he points out that there was a deep division in the household, with David, the father, and Herbert, the son, in conflict

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with Jane, the mother, and Herbert's two sisters. David initiated his son into Auguste Comte's antifeminist religion of humanity; Levy points out that David's understanding of Comte later led him to see the development of corporations as inevitable but also to believe that they must be forced to accept social responsibility. Levy argues that these viewpoints are crucial components of Herbert's position in *The Promise of American Life*. It is from this connection that Levy develops a strong case against Charles Forcey's and this reviewer's argument that Herbert broke from Comtean influence when he went to Harvard.

For Levy, "*The Promise of American Life* was essentially the application of the thought of Auguste Comte to the industrial society of the United States" (p. 131). Praised by Theodore Roosevelt, the book was seen as a philosophical statement of Roosevelt's New Nationalism. And the shy Croly, a writer for architectural journals, suddenly was given the financial backing by Willard Straight, a Wall Street admirer of Roosevelt, to start a journal that would educate elite public opinion in the principles of Progressivism.

The irony of the *New Republic* was that its editor's expectations about an imminent and benign transformation of American life were shattered by World War I. Croly entered the 1920s bitterly disillusioned and searching for solace in visions of a universal Catholic community rescued from the fragmentation caused by the Reformation and in the promises of specific mystics who had followings during that decade. Croly's career as a prophet of Progressivism thus ended ten years before his death in 1930. For Levy, however, Croly's pessimism following 1919 was mistaken "because the 1920s were, in fact, the aberration—a strange interlude in a century that has otherwise largely devoted itself to social experimentation, creation of the welfare state, steady nationalization of American life at the expense of states and localities, organization of workers, expansion of executive authority, attempts to control the economically powerful through government action, aggressive initiatives in international relations, and many other things which Herbert Croly argued as the century opened" (p. 307). Levy, therefore, has not entered into debate with scholarly critics from the Left or Right, who are less sure than he is of the theoretical soundness of Croly's Progressivism and its cultural legacy.

DAVID W. NOBLE
University of Minnesota

ROBERT E. BURKE, editor. *The Diary Letters of Hiram Johnson, 1917–1945*. In seven volumes. New York: Garland. 1983. $750.00.

It is symbolic that Hiram Johnson's obituary appeared on the front page of the *New York Times* on the day the atomic bombing of Hiroshima was reported. He has received less attention than his importance warrants ever since. Three-term governor of California, presidential contender, and senator for more than twenty-five years, Johnson was a major figure for the better part of half a century. Perhaps, as editor Robert E. Burke suggests, this is because the manuscript collections of most other Progressive leaders are at the Library of Congress whereas Johnson's is in California and has been available only since 1966. If so, the publication of these volumes should help correct the situation.

These "diary letters" were sent by Johnson to his two sons and, for a brief period before her death, to his daughter-in-law. They cover the entire span of his senatorial career, from early 1917 until shortly before his death in 1945. Although Johnson probably anticipated that at least portions of this correspondence eventually would be made public, his letters constitute unusually frank appraisals of people and events over the years. Some of the material deals with affairs in California, the bulk of it with politics on a national level and with foreign policy. Space precludes listing all the areas of importance, but the following stand out: Woodrow Wilson's handling of the war and the struggle over the League of Nations, Johnson's response to the depression, and his initial support for, then opposition to, Franklin D. Roosevelt's domestic and foreign policies. With regard to the New Deal, Burke rightly notes, "*How* the Old Progressive turned into a conservative can be documented in these diary letters; *why* he did so may continue to puzzle historians" (vol. 1, p. 63). Johnson's comments on Herbert Hoover, William E. Borah, and FDR, to name a few, are almost always fascinating.

In these letters Johnson himself comes across as somewhat pompous and self-righteous, but he was no Throttlebottom. He is disarming when admitting his failures and inadequacies. He shared the prejudices of most people of his time against minority groups and, as a good Progressive, attributed most of the nation's ills to "the interests." At the same time he was a devoted father and husband, much troubled by his sons' problems and extremely solicitous of his wife's well-being. Often witty, Johnson could also be touching, as when he describes his struggles against physical infirmities during his later years.

These volumes are approximately three-quarter-size photocopy, which contributes to the sense of reading the original documents. Most of the letters are typed; the few in longhand are decipherable. Burke provides a helpful introduction of almost seventy pages that covers Johnson's presenatorial career as well as the years during which these letters were written. Few people would find this collection

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interesting in its entirety, but it provides an invaluable research tool that every college and university library should have. Everyone involved in this enterprise should be congratulated, including those at Garland Publishing, Inc., who made the decision to publish it.

ROBERT JAMES MADDOX
Pennsylvania State University

RONALD CREAGH. *Sacco et Vanzetti*. Paris: Découverte. 1984. Pp. 276. 88 fr.

This book first describes how Nicola Sacco and Bartolomeo Vanzetti migrated as young men from Italy to New England, found quite different patterns of work, and soon became Galleanisti entrenched in the somewhat occult world of Italo-American anarchism. The second section presents the crimes with which they were charged, the trial and other legal proceedings, and the continuing struggle for a favorable judgment. The third part examines the enormous national and international impact of the Sacco-Vanzetti affair. The author, Ronald Creagh, previously published *L'anarchisme aux Etats-Unis* (1981) and *Laboratoires de l'utopie: Les communautés libertaires aux Etats-Unis* (1983).

The biographical sketches of Sacco and Vanzetti are interesting but brief. Good use is made of Vanzetti's published letters to his family, *Non piangete la mia morte* (1962), but little is done with the formidable body of Vanzetti's unpublished letters and unstudied articles. Creagh notes that he does not attempt to cover in depth the case at law or to offer an opinion on the issue of guilt or innocence within the value system and procedures of American criminal justice.

Creagh's placing of the case against the backdrop of anarchist thought and organized activity is, with respect to the United States, fragmentary but fairly proportioned; for Italy it is perceptive and uses some new and significant sources, yielding, for example, a coherent picture of Mussolini's varying attitudes toward the case. For France, the story often leaves the case and mainly explores the rhetoric of the never-ending conflicts among parties of the Left. Some of this rather ponderous context is tedious, but it also embraces important new material; for example, Vanzetti's letter of July 10, 1927, written to the French anarchists six weeks before his death, embodies his significant distinctions among the kinds of "justice."

The book has distracting but not fatal errors. The Supreme Court of the United States did not refuse a new trial; the case was never before that body. Sacco is married to Rosina Zambelli in the text, but to Rosina Ruzzamenti in the index. A. Lawrence Lowell appears correctly as the president of Harvard and as chairman of the governor's advisory committee; he is also wrongly named the governor of Massachusetts in the text and index. More serious is the absence of any meaningful reference to the now available resources of the Frankfurter papers and the Lowell archives. Nor did Creagh consider the work of Nunzio Pernicone, a leading authority on Italian anarchism in the United States and on special aspects of the Sacco-Vanzetti case.

The five-page postface, "Sacco et Vanzetti aujourd'hui," is provocative for both students of the case and historians generally. Creagh points out how, after examining the several ways that ethnic conflict entered into the case, he had to go still farther and to "attempt to discover whether this confrontation of 'mentalities' originated in a clash of thoughts and feelings or whether it had been provoked by a deliberate manipulation of public opinion" (p. 252). Later, he observes how often in our own times legal murder sees its symbolic import fade away or be variously disposed of and how often, "beyond our having become accustomed to such acts, there reigns, unchallenged, a general sense of powerlessness" (p. 254). As Creagh notes, this did not happen in the case of Sacco and Vanzetti. Why, he asks. His search for an answer takes him into a graceful exploration of the world of their day, a world that has largely disappeared but is in measure our parent. This book is well written; particularly stimulating is the lively use of French familiar idiom.

LOUIS JOUGHIN
Chevy Chase, Maryland

MARTIN L. FAUSOLD. *The Presidency of Herbert C. Hoover*. (American Presidency Series.) Lawrence: University Press of Kansas. 1985. Pp. xii, 288. $22.50.

The University Press of Kansas has performed a valuable service in commissioning the volumes in its American Presidency Series. Far from simply a series of works chronicling the events of each presidency, the Kansas series seeks also, according to its editors, to look at how the office of president "developed and operated during the president's tenure," the relationships between the president and "his staff, the executive officers, Congress, foreign representatives, the judiciary, state officials, the public, political parties, the press, and influential private citizens" and to examine the views of the presidency held by its occupants.

This is a tall order, indeed, for the historian viewing any presidency, and certainly the presidency of Herbert Hoover presents more problems than most, for Hoover was a complicated man and his presidency dealt with complex problems. To the

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credit of the editors of the series, they chose probably the only historian able to deal with the Hoover presidency so succinctly (only 246 pages) and yet with such depth and understanding. Martin L. Fausold brings to his work the background of having directed two scholarly conferences on the Hoover presidency, one of which resulted in the publication of *The Hoover Presidency: A Reappraisal* (1974).

The Presidency of Herbert C. Hoover is based on wide research in manuscript and other primary sources and shows the author's familiarity with all of the relevant secondary literature. His description of the influences on Hoover's public philosophy is thoughtful and persuasive, particularly in his examination of the Quaker influence, as is his analysis of that philosophy in a chapter titled "The Corporatist Balance." The extravagant expectations that accompanied Hoover's election to the presidency are described by Fausold, as well as the apparent fulfillment of those expectations during the first months in the White House. The early promise was aborted, however, by the economic slide that began with the stock market crash, and Fausold has written an excellent analysis of the milieu in which Hoover operated in attempting to deal with the unfolding depression—the president's difficulties with the press, with Democrats, with insurgent Republicans, with the Federal Reserve Board, and with businessmen and bankers. Yet, Fausold points out, Hoover was largely successful in getting his antidepression measures put into effect, if only because even his severest critics had little to offer as alternatives.

Despite the unprecedented intervention in the economy that took place during his presidency, Hoover's program did not end the depression, although a slight upturn began in the summer of 1932, nor did it win the support of the public. Hoover's talents as a publicist failed him, and his weaknesses as a politician bedeviled him. To Hoover's credit, however, his successor, Franklin Delano Roosevelt, was no more able, in seven years of peacetime, to end the depression than was Hoover during the three years that he grappled with it.

This is the most important work on the subject yet published, and it is likely to rank as the standard source on the Hoover presidency for years to come.

GARY DEAN BEST
University of Hawaii, Hilo

HUGH ROCKOFF. *Drastic Measures: A History of Wage and Price Controls in the United States.* (Studies in Economic History and Policy: The United States in the Twentieth Century.) New York: Cambridge University Press. 1984. Pp. xi, 289. $29.95.

Economist Hugh Rockoff sets out to describe and evalute American efforts mainly in the twentieth century, to manage inflation through price controls. Although the description of control structures, policies, and methods is extensive, Rockoff's central purpose is to test a hypothesis about inflation control and to provide a solution for the future. This is an intriguing and ambitious book, persuasive at one level but superficial at another.

The book succeeds in its attempt to test the validity of a hypothesis about circumstances in which price control can be effective. The control episodes of the two world wars and the eras of the Korean and Vietnam wars are treated as "natural experiments." The hypothesis is that, contrary to the view widely held among contemporary economists, price controls applied to halt a prospective runaway inflation are not inevitably cancelled by postcontrol catch-up inflation. If the inflationary surge is caused by expectations rather than by monetary influences and if the money supply is kept in check during the period of controls, the net effect will be restraint of prices below the level to which they otherwise would have risen.

For each episode, Rockoff presents data on price behavior and money supply before, during, and after the imposition of controls and from this draws his conclusions. The salient factor in each case is whether, prior to controls, prices were rising faster than money supply, indicating inflation driven by expectations. His overall conclusion is that in every case the application of controls cooled inflation caused by expectations and that, thanks to monetary restraint in all cases but one (that of Vietnam), catch-up inflation did not cancel out the former benefits. His presentation is reasonably convincing though somewhat marred by a curious failure to assess the extent of postcontrol inflation after World War I as carefully as he does in the other instances.

In a parallel fashion the author tests the hypothetical objections often made against price controls: they cause economic inefficiency, their administrative costs are too high, and their evasion is too difficult to suppress without using unacceptable methods. In judging these theses, Rockoff relies first on an evaluation of contemporary assessments and ultimately on the quantitative data he develops. The inefficiency thesis is tested by the author's resort to the index of total factor productivity during control periods; the administrative costs thesis is tested by estimating total bureaucratic costs and comparing them with other governmental services, and the thesis that evasion will be so severe as to require unacceptable enforcement methods and endeavors is examined by assessing the available data on the volume of prosecutions and on sampled public opinion. Since he finds that total factor productivity did not suffer during control episodes, that administrative costs were not comparatively large, and that

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evasion was suppressed by ordinary enforcement efforts, Rockoff is sanguine about the usefulness of price controls in combating runaway inflation if the conditions of his prescription are met and if controls are not prolonged too long.

The book needs to be judged on two levels. It is convincing as an assessment of the formal record of controls and as an explanation of why they were effective in three episodes and not in a fourth. Whether Rockoff's most important conclusion—that his findings were relevant to the inflationary surge of the late 1970s (the book was written in 1977–78)—is accurate is a question best left to economists. It seems evident, however, that the context of the late 1970s, although similar in some respects to the periods when controls were successfully applied (expectations fueled by supply shocks and excessive demand), also contained puzzling differences.

But what of the adequacy of the book as a history of the ideas, intentions, motives, and maneuvers of the actors in these dramas of control? Perhaps the question is unfair since the author's central purpose is clearly theoretical. Yet he spends so much space on descriptive material and offers so many judgments on the origin and evolution of policies that the lack of archival research becomes an embarrassment. A fuller awareness of the literature on relations between government and business, on the evolution of economic thought, and on the long twentieth-century struggle over economic policy between contending groups and ideologies, between statists, liberal corporatists, and antimonopolists, will be required to clarify the role of the price control episodes in the history of political economy in the twentieth century.

ROBERT F. HIMMELBERG
Fordham University

THEODORE ROSENOF. *Patterns of Political Economy in America: The Failure to Develop a Democratic Left Synthesis, 1933–1950.* (Modern American History.) New York: Garland. 1983. Pp. 283. $36.00.

Ten years ago Theodore Rosenof's *Dogma: Depression and the New Deal* placed a capstone—or gravestone—on the argument between defenders of the New Deal's nonideological activism and revisionists who claimed that such a piecemeal approach could not meet the need for radical change. Rosenof tended to seal off the issue by indicating that the New Dealers thwarted themselves because they could not get beyond their own belief in the dogma of free enterprise. Thus, Rosenof left the study of the New Deal mired in impasse, inadvertently lending aid to those who sought to rehabilitate Herbert Hoover as spokesman for real American norms.

Since that nadir in the mid-1970s, some different ways of looking at the past have revived a sense of the New Deal as a far-reaching innovative force. This postrevisionist approach relies on social history and biography in an attempt to balance the intimate and public sides of political figures and movements. As the findings accumulate, the impression grows that New Dealers tended to be strong-minded persons whose rich experiences prompted loyalty to Franklin Roosevelt and rapport with his constituency. Increasingly, it seems plain that the undertow of cultural development controlled the drift of political affairs.

To succeed fully, however, postrevisionism must span the divide separating the new cultural perceptions and the more empirical outlook that still governs political history. Rosenof's study of political economy thus brings him back to the scene at a timely moment and in good company. His is the third book in a laudable Garland series on modern American history that has helped fill the gap between empirical and cultural understanding. In 1979 Steven Kesselman's *The Modernization of American Reform* explained that the New Deal evolved out of a broad front of social and economic thought and beliefs. Dean May's *From New Deal to New Economics* (1981) pinpointed the crucial evolutionary moment by showing how the debate over the recession of 1937 determined the ultimate shape of New Deal convictions and the role Keynesianism was to play in public life. Rosenof extends that grasp by discerning a consensus among the more radically reflective New Dealers and critics to their left on the causes of the depression and, accordingly, on the basis of planning for its cure. Rosenof's emphasis on what persons near the center of affairs believed aligns him with the postrevisionists; his attention to the programmatic action surrounding those beliefs links him with the empirical historians. An especially compelling result of Rosenof's lucidly balanced case is the impression he leaves that the lasting public allegiance to the New Deal as a coherent force was based on something real, even though the consensus on the Left often failed to realize its aims.

But Rosenof takes a step beyond that point. He insists that the consensus on the Left continues to offer the best chance for reform. To him that portion of the New Deal legacy, not the warmed-over Coolidge-Hoover amalgam now in ascendancy, represents the true norm, "an approach rooted in indigenous American ideological traditions" (p. 244). Rosenof's call to follow that coherent approach is the strongest indication to date that postrevisionism can rouse an activist reliance on the New Deal tradition.

ALAN LAWSON
Boston College

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MARK H. LEFF. *The Limits of Symbolic Reform: The New Deal and Taxation, 1933–1939*. New York: Cambridge University Press. 1984. Pp. ix, 308. $32.50.

In popular mythology Franklin Roosevelt's New Deal is remembered as having taxed the rich to benefit the poor. And, although historians have long known better—that in reality most of the revenue raised during the period came from regressive forms of taxation—they have been puzzled by the gap between image and substance. They need be no longer. For in this remarkably fine piece of historical scholarship Mark H. Leff shows that there were really two New Deal tax systems: a "revenue workhorse," featuring regressive levies so masked as to allow coexistence with Populist and redistributionist rhetoric, and a "symbolic showpiece," raising insignificant amounts of revenue but serving the New Deal politically by undercutting and diverting attacks from both the Left and the Right. One became the hidden substance; the other produced the image that still persists.

Leff's book is in essence a well-documented and engagingly presented substantiation of this two-system view of New Deal taxation. In an initial chapter, "Taxing the Forgotten Man," the author focuses on the "revenue workhorse," showing how it emerged from the pre–New Deal heritage; how it was broadened by regressive taxes attached to agricultural adjustment, social security, and the repeal of prohibition; and how revenue demands combined with middle-class resistance to income taxation to sustain the results. In the next three chapters he reconstructs the pre-1937 history of the "symbolic showpiece," noting its origins in a neo-Populist congressional bloc, the initiatives leading to the "anti-rich" and "anti-bigness" taxes of 1935 and 1936, and how the results worked to keep both the neo-Populist and quasi-socialist leftists in Roosevelt's camp while doing little to advance their objectives. And in his concluding chapters Leff explores business's post-1937 obsession with demolishing the "symbolic showpiece," concluding that the battles involved served as a "lightning rod" diverting resurgent business power away from the more substantive New Deal reforms. The "showpiece," he believes, had important implications for the shape and image of the New Deal state, but it did not and was never intended to redistribute wealth downward.

Leff's analysis, which penetrates much that has been a mystery, is on the whole highly persuasive. But some of his evidence suggests that his treatment of the undistributed profits tax as a form of symbolic taxation may need reconsideration. It emerged, as he himself notes, chiefly in response to revenue demands, and it did involve a kind of intervention challenging the very heart of corporate capitalism—its control over the investment process. An argument could be made that business's interest in the undistributed profits tax was grounded in a sound assessment of the realities of power and that business power deployed here eventually served its interests better than power deployed elsewhere.

In providing historical context for his story, Leff also tends to overemphasize the degree of business solidarity on issues other than taxes, and in a few places the complex ambiguities he discerns could be made clearer than they are. But, on balance, this is an exceptionally good book and a major contribution to New Deal historiography.

ELLIS W. HAWLEY
University of Iowa

GEORGE Q. FLYNN. *Lewis B. Hershey, Mr. Selective Service*. Chapel Hill: University of North Carolina Press. 1985. Pp. xv, 386. $24.95.

This is the first major biography of General Lewis B. Hershey. Mirroring the general's own public career, it is also a detailed history of the policy and structural changes that characterized the nation's military manpower procurement system over six decades of the twentieth century. The book, which has been exhaustively researched and documented, includes a useful essay on sources and a thorough index in addition to three sets of photographs and cartoons that help to capture the times through which General Hershey maneuvered the Selective Service System.

The study focuses on Hershey as the manager of national manpower procurement rather than as a developing public personality. After some evocative sections on Hershey's early life (particularly the difficulties of life and career advancement in the peacetime army during the 1920s and 1930s), Hershey recedes as a person and is replaced by a bureaucratic manager enmeshed in the Washington policy process. George Q. Flynn depicts the intragovernmental struggles over Selective Service policies and practices well, although he does not analyze with much subtlety or depth the reasons for Hershey's much-noticed (and quite real) success as a bureaucrat.

The themes of the book, in addition to that of Hershey's own political skillfulness in Washington, are, first, the way in which rising draft-age populations changed the function of Selective Service and then destroyed its public credibility and, second, the broader cultural change in the country over the course of Hershey's life that left him ultimately unable to cope with the resentful protests of the new generation of youth. Although more might have been made of the dated internal ideology, rigidity, and backward-looking determination within the Selective Service—much of which flowed from Her-

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shey himself—Flynn has located his detailed account of policy changes within an appropriate and convincing framework.

Students of the Washington bureaucracy and of Congress, as well as of manpower procurement policy itself, can profit from this account. Future work on the history of the 1960s and the antiwar movement will not be complete without consulting this window on reactions in Washington to the unprecedented manpower surplus and to the protesters' behavior in the face of an unpopular war. The story of efforts to suppress antidraft protest through reclassification and induction, for example, is particularly revealing of congressional attitudes and Lyndon Johnson's political style, as well as poignantly descriptive of Hershey's first major political mistake.

Some annoying excesses of adjectival characterization mar the work, but these may be attributed to the author's desire to enliven what occasionally threatens to become a tedious account of bureaucratic interaction and the evolution of changes in policy, regulations, and practices. Regardless of one's perspective, it is discordant to read of John Foster Dulles "raving" about the worldwide communist threat or scientists complaining "long and loud" about Hershey's seeming unconcern about graduate students in the sciences (p. 205); the same is true of the "deferment scene" becoming "slightly ridiculous" in 1955 (p. 206) and Eisenhower's "adventurous" foreign policy in the same year (p. 207). These instances are not as isolated as one might wish, but they do not really detract from what must be seen as both the leading biography of General Hershey and the best history of changes in Selective Service policy and practice within the political context from 1940 through 1970. The book is thoroughly researched and a distinct professional service.

KENNETH M. DOLBEARE
Evergreen State College

HENRY H. ADAMS. *Witness to Power: The Life of Fleet Admiral William D. Leahy*. Annapolis, Md.: Naval Institute Press. 1985. Pp. xiv, 391. $22.95.

This is probably the definitive biography of Admiral William Leahy. Henry H. Adams has searched the admiral's papers, made full use of his diary, and canvassed associates for information. More effort might have gone into searching secondary works and the papers of other principals, but the picture that emerges here seems complete. The problem is that even the complete picture only dimly reveals the man and his influence. This is partly Leahy's fault: as Adams himself admits, Leahy "tended to carry his reticence to the point of self-effacement" (p. 97). Consequently, since Leahy "wrote to inform but [kept] his feelings out of what he wrote" (p. 346), he contributes little to our knowledge of either the presidents he served or the events he witnessed. Add to this reticence Leahy's formal manner and one has a biographer's nightmare. Try as he might, the author has been unable to breathe life into this man. One is left with the impression that Leahy was honest, cranky, capable, somewhat reactionary, probably influential, yet terribly dull. But, if so, how did he achieve such prominence? Adams's explanation is that it resulted from Leahy's leadership qualities and his ability to cut to the heart of matters, yet these talents are not much in evidence.

The unanswered question concerns the amount of influence Leahy actually exerted. It is true that he was at Roosevelt's and Truman's side, that he went to all the wartime conferences except Casablanca, and that his employers were dependent on his advice. But what decisions did he affect? From the information provided here, his most important roles were as a sounding board, a briefer, a conciliator, a confidant, and a drafter of messages. He hardly comes across as a decision maker or a wielder of great power.

By far the most interesting portions of the book deal with Leahy's career after he stepped down as chief of naval operations in 1939 and began the special assignments that spanned the next thirteen years. He served as governor of Puerto Rico, as ambassador to Vichy France, and as chief of staff to both Presidents Roosevelt and Truman. The most important assignments were the ambassadorship and his work with Truman. At Vichy he performed valuable service and provided substantive reports; he also gained a dislike for Charles de Gaulle that probably did influence American attitudes toward France. Although Leahy and Roosevelt were personally close, it was with the inexperienced Truman that he played his most valuable role. He briefed the new president on the strategic situation in April–May 1945, which was helpful to Truman during the first months of his presidency. Leahy's ardent anticommunism may also have helped set the tone for the administration as Truman would have heard few things sympathetic or conciliatory about either the Soviets or the Chinese from Leahy.

Adams has worked hard on this book: it is well written, well researched, and comprehensive. Yet as a biography it is disappointing, although perhaps any biography of Leahy would be disappointing; taking Gertrude Stein's famous comment about Oakland, California, out of context, it appears that there is no there there.

JAMES LEUTZE
University of North Carolina

KENNETH BAXTER RAGSDALE. *Wings over the Mexican Border: Pioneer Military Aviation in the Big Bend.*

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Austin: University of Texas Press. 1984. Pp. xxi, 266. $24.50.

On the evening of April 11, 1929, a small band of raiders fleeing from both Mexican federal and rebel authorities paid a call at a ranch and trading post operated by Elmo and Ada Johnson in the isolated Big Bend country, some one hundred fifty miles south of Alpine, Texas. As border raids go, it was not much of a confrontation. The Johnsons were able to drive the bandits off without loss of life or livestock. The incident did underscore the need for increased American border security, however, and led to the creation of an emergency Army Air Corps landing field on the Johnson property. Officially established on July 6, 1929, as one of a chain of airfields maintained by the Southwestern Airways Section of the Eighth Corps Area Command, the field at Johnson's Ranch would remain in operation for the next fourteen years.

The remote and isolated landing strip was something of a minor legend in the Air Corps. As one official remarked, "There was just nothing else quite like Johnson's Ranch." The little field was virtually Elmo Johnson's private preserve. As unofficial "base commander," he laid out the primitive runways, constructed the adobe command hut, built the radio station, and provided quarters and food for the pilots passing through the facility.

The renowned Johnson hospitality and the opportunity for good deer hunting in the Big Bend area combined to make the field a popular way station for occasional border patrol missions, a turnaround point for navigational training flights, and a goal for pilots out on "strange field landing exercises" and proficiency flights. As a result, an extraordinary variety of pilots and aircraft passed through Johnson Field during its active life, from 1929 to 1944.

Kenneth Baxter Ragsdale's history of Johnson Field offers some interesting glimpses of the men and machines of the U.S. Army Air Corps during the years between the wars. He introduces us to the future air leaders who visited the facility as young officers. Lieutenant Nathan F. Twining, for example, first touched down at the little strip in 1933 and remained a lifelong friend of the Johnsons. The author's comments on the service aircraft that passed through the field are often fascinating, based as they are on the comments of the pilots who flew them. Ragsdale's descriptions of special operations conducted in the area, such as the autumn "field exercise" conducted by the Third Attack Group in December 1933 (actually a week-long deer hunt), offer useful insight into life in the depression-era Air Corps.

In researching the volume, the author conducted an extensive series of interviews, all of which are catalogued in the thorough notes and bibliography. The well-chosen photographs are a nice complement to the text.

TOM S. CROUCH
National Air and Space Museum
Washington, D.C.

WILLIAM STUECK. *The Wedemeyer Mission: American Politics and Foreign Policy during the Cold War*. Athens: University of Georgia Press. 1984. Pp. x, 177. $18.00.

This short study, really an extended essay, fills a small gap in the growing literature on Chinese-American relations during the turbulent civil war of the late 1940s. William Stueck elucidates the curious decision by Secretary of State George C. Marshall to dispatch General Albert Wedemeyer on a comprehensive "fact-finding" mission to China and Korea at the very time the Truman administration had practically resolved to "write off" Chiang Kai-shek. His explanation, though not novel, is well documented and credible given the author's research in the previously unavailable Wedemeyer papers. Marshall, Stueck explains, had great respect for Wedemeyer's administrative abilities despite his close wartime ties to the Chinese Nationalists. The mission, Marshall hoped, would deflect the demands of the congressional "China bloc" for substantial aid to Chiang, ease the troubled passage of European aid programs through Congress, and commit the administration to nothing. Korea was included on the itinerary as a quick way to bolster the American position there despite the indifference of the Pentagon and of Congress to Korean security.

To Marshall's disappointment, Wedemeyer recommended greater economic aid and tactical military involvement than the administration supported. He also promoted a fanciful scheme to impose a U.N. trusteeship over Manchuria, which would have required a major American military commitment. Fearing the report would only embarrass the beleaguered Nationalists and fuel pointless domestic debate, President Truman and Marshall buried the September 1947 report under a label of secrecy until its partial publication in the 1949 White Paper.

Stueck cogently traces Wedemeyer's personal evolution from being a "centrist" and team player to being a mouthpiece for the conservatives in the "who lost China" debate after his retirement from the army in 1951. Ultimately, the author concludes, the administration was correct in ignoring Wedemeyer's recommendations while Republican critics behaved shamelessly in using the report for partisan ends. Stueck has crafted a slim but professional case study that should prove the last word on the

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Wedemeyer mission. The book also contains a useful annotated bibliography.

MICHAEL SCHALLER
University of Arizona

PETER L. STEINBERG. *The Great "Red Menace": United States Prosecution of American Communists, 1947–1952.* (Contributions in American History, number 107.) Westport, Conn.: Greenwood. 1984. Pp. xiv, 311. $35.00.

Two difficult problems face any author who attempts to write something of significance about America's postwar anticommunist politics: first, so much has already been written that it is sometimes hard to say anything original; second, so much of the archival record that might permit further investigation remains classified or has been destroyed. In this study of the Smith Act prosecutions of American Communist party leaders during the Truman years, Peter L. Steinberg unfortunately fails to solve either of these problems.

The author has read widely in the secondary literature and has examined a large number of important archives. Yet, with the exception of a handful of FBI documents pried from a reluctant Department of Justice and several interviews with former Communist party leaders, he has uncovered no significant new sources of evidence. Nor, with two important exceptions, does his book offer much that is not already familiar to historians of the postwar era. Drawing on the FBI files, he presents a provocative, if necessarily incomplete, account of J. Edgar Hoover's Machiavellian efforts to maneuver a more or less reluctant Justice Department (and Truman administration) into the prosecution of American Communists under the law enacted in 1940. Drawing on the letters and interviews with former Communist party leaders (including, apparently, his father), Steinberg provides a valuable discussion of the debate over legal strategy within the party and of the mostly ill-conceived decisions adopted as a consequence. But on the other issues encompassed by this study historians will learn relatively little that they do not already know. The Smith Act prosecutions have been more fully examined by Michael Belknap; the role of Hoover and the FBI has been more thoroughly explored by Athan Theoharis, Frank Donner, and Kenneth O'Reilly; the cold war politics of anticommunism has been exhaustively detailed in dozens of books; and the tangled and sometimes self-destructive course of the American Communist party has been traced in a variety of recent books, articles, and memoirs. In addition, Steinberg's book is unfortunately not particularly well written. It is poorly organized, overuses the passive voice, and employs an overabundance of clichés.

We have by no means exhausted the meaning of those terrible times. Indeed, given the chill winds now blowing through our contemporary culture, we need more than ever to ponder their significance. This book contributes only minimally to this undertaking, however. What might have been two or three solid journal articles has become instead an exercise in redundancy.

ROBERT GRIFFITH
University of Massachusetts

JONATHAN G. UTLEY. *Going to War with Japan, 1937–1941.* Knoxville: University of Tennessee Press. 1985. Pp. xiv, 238. $19.95.

In his analysis of American decision making prior to Pearl Harbor, Jonathan G. Utley argues that persons interested in the Pacific war will be misled if they study only high-level policy decisions or if they assume that an action taken in Washington necessarily reflected a decision made at the top. He sees Franklin D. Roosevelt's foreign policy establishment as "a snake pit of influential leaders and faceless bureaucrats working at cross-purposes, striking deals, and . . . employing sleight of hand . . . to move the nation in the direction each thought most appropriate" (p. xiii).

The central figure throughout the book is Roosevelt's secretary of state, Cordell Hull, who did not want to abandon American interests in the Far East but who also refused to support any response that might lead to a confrontation with Japan. He therefore waited for the situation to improve while it slowly but steadily deteriorated. Following the outbreak of war in Europe, new agencies and administrators arose in the nation's capital. This spelled trouble for the future because these newcomers "were not adequately integrated into the foreign policy establishment. They constituted a gray area not under Hull's control yet still affecting the implementation of national policy" (p. 95).

As 1940 passed into 1941, the bureaucratic battles continued with lower-level officers in various agencies often working at cross-purposes with Hull. Ironically, however, the secretary's policy of patience was substantially altered not by an outsider but by one of his own subordinates, Assistant Secretary of State Dean Acheson, supported by like-minded colleagues in the Treasury and Justice departments. Together they transformed an executive order freezing Japanese assets in July 1941—a policy Roosevelt had apparently regarded simply as a final warning—into something far more serious: a program of economic retaliation that would bring Japan to its knees. Just when the president found out that a de facto embargo had been imposed is not clear. In any event, no changes were made. In

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seeking to explain FDR's willingness to maintain the embargo, Utley suggests that a return to Roosevelt's original idea "would have sent the wrong message to Tokyo and reinforced the position of Japanese hard-liners, who claimed that the United States would give in" (p. 156). Another possibility is that both Roosevelt and Hull had in the meantime decided that stringent measures might be the only way, short of war, to bring Japanese leaders to their senses.

To Utley, Hull emerges in the autumn of 1941 as a man "trapped in a diplomatic crisis from which he saw no escape" (p. 175). By abandoning a modus vivendi proposal that was under consideration in November, "Hull made a decision," Utley believes, "that, in effect, chose war with Japan rather than a collapse of China" (p. 178). An argument can be made, however, that China would not have collapsed even if the proposal had been sent to Tokyo. Also, concern over China's reaction was only part of the reason why Hull turned away from an idea he had earlier been willing to pursue. Finally, conciliatory efforts by Washington at that particular moment might have led Tokyo to think that the U.S. would back down rather than fight.

This book is clearly and forcefully written, so forcefully that some readers may wish, as I did, to qualify a number of Utley's assertions. The book is best in those sections in which the tensions and pressures within the American decision-making establishment are described and analyzed. This feature of government needs to be taken into account by anyone interested in the way in which foreign policy is formulated. At times, however, the author pays so much attention to what was happening in Washington that he ignores what was taking place in Tokyo. The Japanese challenges that triggered the reactions not only of Hull and other top American leaders but also of the "faceless bureaucrats" who were at work behind the scenes are not clearly defined. No comparisons are made with Japanese counterparts. Thus, the picture that emerges is lopsided. This is particularly noticeable toward the end of the book where the coverage of developments is very selective.

Hull was human. Like others in Washington (and Tokyo) he was capable of making mistakes. But he was only one of the actors in a complex historical drama. A balanced evaluation of his role and of the part played by the Roosevelt administration cannot be achieved unless the performances of the other members of the cast, Japanese and American, are simultaneously scrutinized. Only if we do this will we have a chance of ultimately understanding how war came in the Pacific.

R. J. C. BUTOW
University of Washington

LEE KENNETT. *For the Duration: The United States Goes to War, Pearl Harbor—1942*. New York: Charles Scribner's Sons. 1985. Pp. ix, 243. $15.95.

This book by Lee Kennett is a brief examination of the United States in the first six months of World War II. Consisting of a series of thematic chapters, it chronicles the changes that occurred throughout American society after the Japanese attack on Pearl Harbor in December 1941. Based on newspapers, archival materials, and secondary works, the book is more a summary than an intensive survey of the home front in the war.

Kennett deals with a variety of problems, ranging from the introduction of censorship to civil defense. He describes developments in the arts and outlines the formation of information policy. In general, his account is clear and covers well the major issues of the day. In telling his story, he weaves in the personalities who played important roles at home, including such figures as Franklin D. Roosevelt, George C. Marshall, Fiorello La Guardia, and Archibald MacLeish.

The book, as Kennett acknowledges at the outset, is hardly a comprehensive history. Toward the end, he notes once again that he has been "looking at only the thinnest 'slice' of the nation's life" (p. 200) with his survey of the start of a struggle that would drag on for three more years. Yet that disclaimer does not preclude criticism of the choice of period. Although Kennett is interested in capturing the mood of the nation at one brief moment, it is never quite clear why the account ends when it does. Kennett begins to talk about developments that were important—problems of industrial mobilization, questions of what to do about the Japanese in the United States—but, given his own bounds, he is unable to carry the account further in any area, and so one is left with a sense of a story that is incomplete.

Kennett does have a good eye for detail. With an almost anecdotal approach, he brings the war down to a personal level as he relates how Rotarians from Kodiak, Alaska, vowed to let their beards grow until the war was won and how people wanting more toothpaste had to turn in an empty tube. The book is filled with pithy quotations culled from the sources that illustrate the broader themes in a dramatic way.

This book does more than spark a feeling of nostalgia. It is a fair and even-handed treatment of many of the important issues of the war. Yet it adds little to the work of Richard Polenberg, John Morton Blum, or even Geoffrey Perrett, who have contributed most to the growing body of literature about the war. The book stands as a supplement to existing accounts but hardly takes their place.

ALLAN M. WINKLER
University of Oregon

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ALAN DOWTY. *Middle East Crisis: U.S. Decision Making in 1958, 1970, and 1973.* (International Crisis Behavior Project.) Berkeley and Los Angeles: University of California Press. 1984. Pp. xiv, 416. $42.50.

Alan Dowty's well-written and well-researched comparative study of United States decision making during the 1958 Lebanon crisis, the 1970 Jordanian crisis, and the 1973 Yom Kippur War will not interest all diplomatic historians. His volume is one in a series from the International Crisis Behavior Project, which seeks to understand how diplomats in crises since 1938 have coped with stress and how they have made their policy choices from among perceived options. The authors of books in this series are less concerned with historical narrative than with eliciting general theory from case studies.

Dowty presents sixty-five hypotheses—almost all of which are part of the common project framework—that he tests during precrisis, crisis, and postcrisis periods. Most deal with the impact of stress on images and decision-making structures during the crises. Previous analyses, especially those dealing with World War I, have emphasized the negative effects of stress on decision makers. Somewhat surprisingly, but convincingly, Dowty discovers that stress sometimes produced positive results in terms of rational and effective coping strategies. Further, he concludes that, although American diplomats' perceptions were often flawed during his three crises, the decisions they made under stress were no less correct than those made during more normal periods. Immediate American aims were, at least, achieved in all of the crises.

Historians not excited about such theoretically oriented conclusions will nevertheless find much of value in Dowty's presentation. In the first place, he conducted extensive interviews with major actors in the 1970 and 1973 crises including William Colby, Richard Helms, Melvin Laird, Thomas Moorer, and Joseph Sisco. Skillfully employing their recollections, as well as those of other well-placed aides in the National Security Council, Department of Defense, and State Department, Dowty turns up useful information about who said what to whom in what contexts during specific periods. The Lebanon crisis, for which he was unable to obtain materials under the Freedom of Information Act and for which he apparently did no interviewing, is less of a contribution.

More generally, Dowty's analyses of decision making and the hypotheses that he and his colleagues are confirming and disproving may be helpful to scholars who traditionally examine one discrete crisis in their monographs. At the same time, historians will object to the way Dowty, a political scientist, sacrifices the chronological narrative to the rigors of his analytical framework. In addition, at times he tends to accept too literally the published statements of Dulles and Eisenhower and Kissinger and Nixon as indicators of their private perceptions of international relations, their overall psychological frames of reference, and their operational codes. Finally, most readers will encounter difficulty in trying to isolate the findings of importance amid the sixty-five closely related hypotheses tested in three different crises.

In *Middle East Crisis* Dowty effectively uses historical data to enrich decision-making theory. Along the way, he adds to our knowledge of the three important crises he has chosen to analyze.

MELVIN SMALL
Wayne State University

EUGENE BROWN. *J. William Fulbright: Advice and Dissent.* Iowa City: University of Iowa Press. 1985. Pp. x, 171. $22.50.

In the 1950s and 1960s Senator J. William Fulbright, Democrat of Arkansas, was an important voice in American foreign policy. A member of the Senate after 1944 and chairman of the Senate Committee on Foreign Relations after 1959, Fulbright played a role of increasing magnitude in that chamber's deliberations. Fulbright was the antithesis of what most observers thought of as Arkansas's self-image. The state's symbol is a ranting razorback, whereas Fulbright was urbane and bookish, witty and caustic, innovative and profound. He was often a paradoxical figure, but the voters of Arkansas kept sending him to Washington and took great pride in the national and global attention he won as the Senate's foremost authority on foreign affairs.

Eugene Brown outlines two main goals for this well-researched and well-documented study of Fulbright's public career. First, he attempts to discover the elements of continuity and change in Fulbright's views on American foreign policy. Second, he seeks to discern the relationship between Fulbright's ideas and those of the nation at large. The author takes a forthright and retrospective look at Fulbright's tenure of more than three decades of public life and correctly concludes that it "affords us to take the measure of a genuinely interesting man" (p. 1).

The book reviews Fulbright's life and career as a former Rhodes scholar, university president, and one of the United States Senate's most illustrious members. The author's thesis is that Fulbright's beliefs about America's international role were quite similar to the dominant views long held by the mass public. Fulbright's "fame as a foreign policy critic has severely obscured the highly conservative nature of his dissent, which has typically dealt with matters

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of means and style, rather than fundamental ends" (p. 3).

Of special importance to the author are Fulbright's views on the proper extent of presidential authority in foreign policy. Before 1965, Fulbright generally favored giving the president a relatively free hand in foreign affairs. He held that Congress was not effectively organized to conduct foreign policy because of its tendency to engage in negativist actions. The weakness of congressionally dominated foreign policy led Fulbright to oppose it, even though he regarded the intervention in the Middle East in the late 1950s and the Bay of Pigs invasion in 1961 as major executive errors. After 1965, Fulbright came to believe that the president had to be restrained by Congress, and especially by the Senate. What Fulbright regarded as Lyndon Johnson's deception in connection with the Gulf of Tonkin crisis of 1964 and the invasion of the Dominican Republic in 1965 were major turning points in his thinking.

The author provides an excellent description of one of the most complex and interesting politicians of his time. Brown concludes that Fulbright, by his dignified example, "legitimized foreign policy dissent for a generation seared by the tragedy of Vietnam. Without him the nation might have forgotten that love of one's country and forceful dissent against its policies are not exclusive tendencies" (p. 122). Fortunately, Fulbright was prolific in his advocacy and left behind a huge record of interviews, speeches, articles, and books for all to ponder. Few members ever used the Senate as a public forum better than Fulbright. Even fewer were as good or as decent a public servant as this gentle giant. The value of this book is that it clearly shows that the nation needs a Fulbright, now and always. We miss him.

ARTHUR F. MCCLURE
Central Missouri State University

KATHLEEN J. TURNER. *Lyndon Johnson's Dual War: Vietnam and the Press*. Chicago: University of Chicago Press. 1985. Pp. ix, 358. $25.00.

This detailed study of Lyndon Johnson's problems with the media is organized chronologically from his prepresidential years to 1968. Kathleen J. Turner was trained not in history but in communications, and she emphasizes rhetoric and the preparation of speeches (one entire chapter is on the evolution of Johnson's Johns Hopkins speech of April 7, 1965). What should be of most interest to historians is Turner's extensive research at the LBJ Library in Austin (with 34 million documents, the largest of the presidential libraries). The text of 254 pages is accompanied by 65 pages of notes and 23 pages of bibliography.

Her argument that Johnson's failure to communicate through the mass media was intrinsically related to the failure of his Vietnam policy is well documented. Johnson's problems with the media began before Vietnam. His political style, the famous "Johnson treatment" that he developed in his early years as a congressman and senator, emphasized informal personal persuasion out of the public eye. When he became president, he found this style to be incompatible with the demands of the modern mass media. His spontaneous and unorthodox press conferences were difficult to cover, especially with television cameras. His cowboy crudeness contrasted sharply with the Camelot sheen previously projected onto the presidency by the media. The "Texas joyride" story in *Time*, pictures of Johnson lifting his beagles by their ears, the raising of his pajama top to reveal to the world his gallbladder scar—such images were not "presidential." The credibility gap originated not with Vietnam but with Johnson's misleading the press about the size of the 1965 budget in his first press conference. Even before he lied to Congress and the American people about the DeSoto patrols that provoked the Gulf of Tonkin incident, Johnson was deceiving the press about travel plans and was making unannounced trips to his Texas ranch.

Vietnam presented Johnson with his most difficult communication problem. By waging a limited war, he did not mobilize the country for victory. Unsure himself of America's war aims, he was vague and contradictory in his public explanations; as criticism mounted, he became defensive and vengeful. Turner explores the growing preoccupation with the "image crisis" through the memoranda of aides such as Jack Valenti and Robert Kintner. They struggled with ways to improve the presidential image and occasionally succeeded. Johnson's November 17, 1967, televised press conference, in which he used a lapel microphone to liberate himself from the podium ("Big Mother"), was one of his best. Johnson, however, always reverted to his old form: he discarded the lapel mike and retreated behind Big Mother; he renewed his attacks on perennial critics such as Katherine Graham's *Newsweek*; he spoke, not jokingly, of chloroforming reporters.

Although the emphasis is on Johnson, Turner finds that the media shared responsibility for the Vietnam problem. The press failed to question fully and probe the Tonkin incident. Many stories about Johnson were biased and exaggerated; his "grandfather at the Alamo" claim was repeated long after a correction was issued by the White House. Turner follows the interpretation of Peter Braestrup that the media created a "disaster" image of the Tet

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offensive out of proportion to the military reality of the attack.

By examining the relationship of the president and the media in the context of the specific issue of Vietnam, Turner shows that in our era the ability of a president to communicate policy through mass media is just as important as the ability to make and execute policy.

STEVEN E. SCHOENHERR
University of San Diego

CAROL E. HOFFECKER. *Corporate Capital: Wilmington in the Twentieth Century*. Philadelphia: Temple University Press. 1983. Pp. viii, 294. $24.95.

This book is a valuable and spirited, although narrowly focused, account of the highly charged political events of the 1960s and 1970s in Delaware's largest city. Carol E. Hoffecker clearly cares deeply about the events she describes. She declares that she intends to contribute to the history of community power, but her real intention, it becomes clear, is to evaluate recent government and corporate policies in Wilmington and to persuade the city's corporations, and even more its suburbs, to take a more generously inclusive view of the larger community in which they live.

In her central chapter, "When We Needed Unity the Most: 1960–1968," Hoffecker offers a detailed, dispassionate discussion of economic decline and racial tensions. Patiently recounting the efforts of government officials and agencies, church and business organizations, activist social workers, and increasingly frustrated black leaders, she shows that too little was done too late to ameliorate the city's worsening social and economic conditions. Eight years of noisy and ineffective effort to revive Wilmington disintegrated on April 9, 1968 (the day of Martin Luther King's funeral), when some comparatively mild civil disturbances gave the state's conservative Democratic governor a pretext to send the National Guard into Wilmington's black neighborhoods for a nine-month occupation.

New, more effective redevelopment efforts followed the searing events of 1968. In sometimes dizzying detail, Hoffecker recounts urban renewal initiatives by successive "liberal" mayors, court-ordered integration plans that repeatedly rearranged the boundaries of school districts within the city and its suburban area, and battles over roads, hospitals, and parks. Although she finds it difficult to put these recent events into perspective, she has produced one of the first careful historical accounts of the local impact of a greatly expanded federal government.

Hoffecker believes that local decision makers largely control the flow of events in their communities. Decisions made in Wilmington, she argues, rather than pervasive social and economic forces or state and federal policies, left the city's poor without jobs, disrupted retail and housing markets, and upset the once-distinguished public school system. She also argues that power became more widely dispersed in the city during the twentieth century, as the Du Pont family lost much of the local influence it had long enjoyed.

Both of these arguments fit the facts that Hoffecker presents; they make her book much more than the "chronicle" promised by the dust jacket and will attract the attention of other historians. Yet they have a somewhat casual quality. Hoffecker herself shows that Wilmington's fate was shaped not only by local decisions but also by changes in industry, transportation, housing, and retailing that were national in scope. And she emphasizes the unsettling impact on local decision makers of a newly powerful but unpredictable federal government. Because she treats the years before 1960 simply as a prelude to the traumatic events of the following decade, she does not really demonstrate that the Du Ponts dominated local affairs in earlier years. Hoffecker's arguments might well stand up to a more rigorous test; the sympathetic reader can only regret that she did not employ one.

DAVID C. HAMMACK
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ARTHUR J. VIDICH and STANFORD M. LYMAN. *American Sociology: Worldly Rejections of Religion and Their Directions*. New Haven: Yale University Press. 1985. Pp. xiii, 380. $30.00.

The misleading and inappropriate subtitle of this book anticipates the larger interpretive problems in Arthur J. Vidich and Stanford M. Lyman's study of American sociology. For, in the large gallery of writers reviewed in this book, only Lester Frank Ward embraced the fervid kind of secularism that would constitute a worldly rejection of religion. Vidich and Lyman attempt to show that the restating of inherited but fading Calvinist values provided most of the other writers with a new "scientific" language that gave sociology its moral urgency and its distinctively American spirit of progress and uplift. In their concluding chapter the authors summarize: "Sociodicy was begun in order to prove that the Christian vision was not an illusion. The professor of sociology had the tasks of both the theologian and the minister" (p. 282).

The governing theme of this work—the appropriation in secular formulations of the theological and ethical norms of Calvinistic Protestantism by

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American sociologists—is evidenced by several chapter titles: "Capitalism and Calvinism: William Graham Sumner," "Saving the World: Social Ethics and Applied Psychology," "Political Economy, Puritanism, and Social Systems," "Secular Evangelism at the University of Wisconsin," and "Towards Moral Brotherhood: Robert E. Park and the Chicago School." The authors are always quick to read from the sociological texts back into the religious outline of the Protestant intellectual system that preceded them. But in doing so they fall repeatedly into a facile and naive mode of intellectual history. They attribute to their writers an overstated or assumed self-consciousness about their efforts in advancing sociology as a surrogate of religion. Thus, in summarizing Hugo Munsterberg: "The Puritan concept of work as a calling for the glory of God had been transvalued into work as a calling for the glory of the work group" (p. 98). On Franklin H. Giddings: "Giddings's evolutionism, which began with brute, sympathetic, involuntary association and ends with rational, voluntary coming together, is a substitute for the means by which a religious community is formed" (p. 109). Or even on Ward: "Thus Ward changed the foundations of the original Holy Commonwealth into a secular vision of a brotherhood of the educated, replacing a covenant of faith with a covenant of knowledge" (p. 27).

The authors, who frequently employ terms like "substitutes," "replaces," and "parallels," need to give much greater scrutiny to the meanings of those words. An axiomatic quality about the thesis of this work recurs throughout, albeit mostly in the early portions. One wishes that Vidich and Lyman had asked what were their subjects' early experiences with religion and how these writers moved into secular habits of mind. Never do Vidich and Lyman offer any evidence that these men underwent an ordeal of transition from the theological to the scientific frame of mind, and in most cases an explicit encounter with the rhetoric and ideals of Puritanism is assumed rather than demonstrated. When Vidich and Lyman assert that Puritan ideas about the unregenerate gave sociology its concept of deviance, are we to conclude that the modern notions would have been different or nonexistent apart from the Puritan background?

These problems of interpretation do not themselves discredit the authors' thesis. American sociology is replete with the language of Protestantism, and unquestionably many of its tenets are the secular variants of inherited religious views of life. The authors make this pattern clear in some convincing and useful chapters, especially those dealing with Giddings, Talcott Parsons, and Albion Small. In these sections the evidence is more carefully marshaled and more concrete; the authors are not left to apply a priori a mechanistic framework to their subjects.

J. DAVID HOEVELER, JR.
University of Wisconsin, Milwaukee

JOHN H. STANFIELD. *Philanthropy and Jim Crow in American Social Science.* (Contributions in Afro-American and African Studies, number 82.) Westport, Conn.: Greenwood. 1985. Pp. xii, 216. $29.95.

A recent article entitled "Questioning the Science in Social Science, Scholars Signal a 'Turn to Interpretation'" in *The Chronicle of Higher Education* (June 26, 1985) points out the problems and pitfalls of the social science investigator. Part of this "turn to interpretation" has come from a realization that the data to be studied in the social sciences differ significantly from those of the natural sciences, leaving objectivity a distant star for social scientists. This has resulted in an image of social scientists, particularly sociologists, wandering outside their traditional "modeling" of social laws and patterns, endeavoring to write social history. The results have been mixed. Paul Starr's *The Social Transformation of American Medicine* (1982) is a good example of how well the sociologist can bridge disciplines to write a solid piece of interpretive history; John H. Stanfield's effort is indicative of the problems that ensnare the unwary.

In his book Stanfield builds a series of straw issues around a potpourri of sociological jargon, newly constructed terminology, oversimplification, and case studies in order to establish "the production of racial inequality in social scientific knowledge" (p. 3). Although he admits that this issue has been addressed by other authors, some of whom he studiously ignores, he criticizes them for promoting "internalistic perspectives that reify the race relations of social science knowledge production from critical societal conditioning factors" (p. 3).

The heart of Stanfield's study is his examination of the patrons and officers of philanthropic organizations—such as the Julius Rosenwald Fund, the Carnegie Corporation, the General Education Board, the Laura Spelman Rockefeller Memorial, and the Phelps-Stokes Fund—who supported social scientific race relations research. He interprets this activity as an "elite tool" for both creating and then maintaining the Jim Crow social structure of late nineteenth- and early twentieth-century American society. As donators and managers of large research funds, these individuals constructed "an elite status culture which produced knowledge as a means of social control" (p. 7). The research produced with their funding, argues Stanfield, occurred within a "network of financiers, researchers and workset-

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tings" that preserved a social structure buttressing the ritualized race etiquette of Jim Crow and the cultural hegemony of Anglo-Saxon elite status groups, bourgeois control, and corporate capitalism (p. 7). Because of this, radical alternatives were sidetracked and only "safe" researchers (and their solutions) were supported.

Although portions of the book are well written and thoughtful, the author is overly repetitive, lapsing frequently into presentism and overstatement, ignoring completely the nature-nurture arguments that dominate much of the literature, and denigrating the ameliorative and pragmatic aspects of the liberal tradition in America. Moreover, he makes no mention of Populism, Progressivism, the depression, or the New Deal; it would appear that the social scientific paradigm requires no such context. Finally, the last chapter, which is intended to provide the theoretical context, instead lists assertions that should have been established earlier. One is forced to remember the old saying that sociology is the study of those who do not need studying by those who do.

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California State University, Long Beach

CANADA

DONALD R. BEER. *Sir Allan Napier MacNab*. Hamilton, Ont.: Dictionary of Hamilton Biography. 1984. Pp. xii, 491.

Canadian historians have long been enamored of biography. The appearance of a lengthy volume on Allan Napier MacNab suggests the continued vitality of that tradition. Although MacNab sought, as he said, to be "No. 1" (p. 13), he was, as Donald R. Beer recognizes, something less than that. Yet, Beer argues, MacNab did assume considerable significance, a fact seldom acknowledged by Canadian historians, who are unduly influenced by "a relatively narrow liberal tradition" (p. ix).

And, Beer maintains, MacNab did achieve. Elected to the Upper Canadian assembly, he became a spokesman for the economic interests of the Hamilton area and a leader of the moderate Tories. His role in the suppression of the rebellion saw him at the pinnacle of his career. The rebellion had an enormous impact on him, and for years afterward he saw a stark dichotomy in political life between those who were loyal and those who were disloyal. The events of 1849, however, led him to recoil from extremist positions and to attempt to fashion a moderate party. Long interested in railroads and a moving force behind the Great Western, he declared that "all his politics were Railroads" (p. 275). In fashioning a developmental party, as he did in 1854, he was aided by John A. Macdonald, who was as much indebted to him as he was to W. H. Draper for the nature of his conservatism. Although MacNab hung on to the premiership too long and was deposed in 1856, he achieved much while in office.

Clearly, Beer's well-written and meticulously researched biography is a sympathetic one that provides important new insights into the politics of the 1830s, 1840s, and 1850s and into the byzantine world of railroad promotion. Some may wonder, though, how it was that MacNab's influence as premier has hitherto been understated when he headed only a minor department and played little role in the passage of the two major legislative items of his administration: settlement of the clergy reserves and abolition of seigneurial tenure. Others may doubt Beer's analysis of MacNab's economic dealings. Evidently, "the gallant knight" (p. 146) did not always have his eye on his bank balance, often evincing an "unbusiness-like willingness to use commercial enterprises for non-commercial ends" (p. 69). "Simple economic interest" (p. 220) provides insufficient explanation of his actions. He was not as unscrupulous as some (notably, Peter Baskerville) have maintained, although after 1850 his standards, admittedly, became more flexible. In explaining MacNab's sharp financial practices, Beer points to "the grey, uncertain areas of contemporary ethics" (p. 404). Those who find this argument unconvincing or distasteful will likely also find hard to understand how MacNab could have had a "reputation for selfishness, corruptibility, and abuse of patronage" (p. 300) and yet shortly thereafter have been regarded as a check on corruption in the cabinet (p. 465 n. 450). This is something of a contradiction, but Beer sees MacNab himself as a contradiction, and not only in financial affairs where he demonstrated both reckless idealism and calculated self-interest.

Beer's excellent, provocative study is of the public man, and MacNab's public life was long and varied. The documentation for it is as rich as that for his private life is thin. This may be, in part, why he appears to be such an enigma. And it may be that if Donald Beer knew him better he would like him less. But probably not. In 1838 MacNab received a copy of an address carried by resolution in the Upper Canadian assembly praising his services in the rebellion. Having eagerly sought glory and having found it, he scrawled on the back of the document, "Not worth a fart" (p. 147).

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P. B. WAITE. *The Man from Halifax: Sir John Thompson, Prime Minister*. Buffalo: University of Toronto Press. 1985. Pp. ix, 547. $37.50.

Sir John Sparrow Thompson owes his tiny niche in Canadian history to his central position among the four prime ministers who struggled to salvage Sir John A. Macdonald's legacy of a divided country and a disintegrating party. Thompson was among the first of those able and seemingly incorruptible lawyers who emerged from the muck of Nova Scotia politics to add dignity to Canada's capital. It seems appropriate that another of his caliber, Sir Robert Borden, was Thompson's heir at the Nova Scotia bar.

For Thompson, as for so many able, ambitious North Americans, the law was the escalator from lower-middle-class drudgery to power and faint glimpses of affluence. Whether as a Halifax alderman, as a member of the Nova Scotia legislature, or, briefly and unhappily, as the provincial premier, Thompson saw politics as no more than a means to the judgeship he coveted and merited. Raised a Methodist, he converted to Catholicism, P. B. Waite suggests, because it possessed the ordered certainty Thompson admired in the world of law. His most important contribution to national life came not in his two frustrating years as prime minister but in his labor to bring forth a Canadian criminal code in 1892.

Catholicism, as much as the law, shaped Thompson's life. Too high-minded to enjoy constituency politics, he could depend on Bishop John Cameron to help deliver most of the votes he needed from his Antigonish electorate. His new religion won him a marriage that might almost have been made in heaven. Annie Affleck strengthened her husband's faith and stirred him out of the backwater of a Halifax law office and the slumbering security of the Nova Scotia bench. Her influence, Waite argues, underpinned Thompson's wholly unfashionable respect for the rights and potential of women. The prime minister's patronage of the National Council of Canadian Women won him few friends among his fellow Tories.

Thompson's Catholicism became a special burden when Macdonald drew him reluctantly into federal politics in 1885. The Protestant bigots who attacked the "pervert," Waite insists, were a tiny, fanatic minority that attracted some leading Conservatives and frightened others. Macdonald's long career had left his party in the hands of old, venal, and self-serving men. As minister of justice, Thompson was one of the few exceptions, but neither his temperament nor his faith made it easy for him to wade into the nasty battles that pitted bigots and opportunists against his coreligionists in Manitoba and the Northwest.

Instead, Thompson turned to work fitted to his skill and style. In the interminable fisheries and pelagic sealing negotiations that bulk so large in the diplomatic archives and so small in the history books, Thompson was Canada's ablest negotiator. Among Waite's many useful contributions is his success in reducing so much material to a persuasive and often witty synthesis.

Thompson's bargaining skill and his delight in the pleasures of Paris in the summer of 1893 were no compensation to colleagues in Ottawa: a Canadian prime minister must be bound by the turgid preoccupations of domestic affairs. Perhaps Thompson could have adjusted, but he was denied the chance. On December 12, 1894, in the midst of installation as an imperial privy councillor at Windsor Castle and almost in the presence of Queen Victoria, the corpulent Canadian politician collapsed and died.

Thompson lived and died well. Among Waite's illustrations is a rare photograph of this Victorian gentleman, surrounded by family and political cohorts, positively convulsed with laughter. Others struggle in vain against the convention of solemnity; even the massive Lady Thompson betrays a ghost of a grin. Waite's Thompson is a man of flesh and blood. Both the book and the man bubble with humanity. Waite may have made Thompson's historical niche no larger, but he has made it warm and comfortable.

DESMOND MORTON
Erindale College
University of Toronto

ROBERT BOTHWELL. *Eldorado: Canada's National Uranium Company*. Toronto: University of Toronto Press. 1984. Pp. xiii, 470. $24.95.

A company history is often a sign of trouble. It frequently signifies the passing of a generation of managers, the need to clean up the company image after a corporate catastrophe, or the loss of institutional memory. Increasingly, corporate histories are now being written as part of a long-range planning process.

This book by Robert Bothwell is an example of all of the above. The men who guided the company through World War II and excitement of the cold war are quickly disappearing and quite naturally want their contributions remembered. Recently the government-owned company was embarrassed by well-founded allegations that it ran an international uranium price-fixing cartel. It is getting hard to remember why the company had to be nationalized in the first place and even harder to justify continued public ownership in a demilitarized, mixed enterprise setting. A pointed company history is as good a way as any of honoring founders, burnishing

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the company's reputation, remembering why, and getting the management goal of private ownership onto the public agenda.

This task is normally given to journalists, but in this instance the company commissioned Bothwell, an experienced professional historian. It was an inspired choice. He charted the broader contours of twentieth-century Canadian political history in an earlier biography of C. D. Howe and a textbook survey. A background in international relations gave him mastery of the broader strategic and diplomatic considerations of the uranium industry. He obviously learned a good deal about geology, chemistry, mining engineering, finance, and monopsonistic markets on the job. The result is a book of fascinating richness and detail, which ranges confidently from the high politics of nuclear weapons to the low dealings of speculators, from cabinet room through boardroom to bunkhouse and mineshaft and over all the matters in between.

Eldorado began in the late 1920s as a speculative radium venture. World War II wiped out the radium supply business but suddenly created an extraordinary demand for one of the company's by-products, uranium, urgently needed for the combined British-U.S. nuclear research programs. Pushed by the British and Americans, the Canadian government secretly conspired to gain control of the company to ensure an uninterrupted supply at maximum output to American weapons makers. The company became, in effect, a compliant Canadian supply arm of the U.S. Atomic Energy Commission, which reciprocated by buying everything produced at generous prices until its stockpile was filled to overflowing at the end of the 1950s.

Bothwell presents the narrative in a mildly ironical style and at a level of detail that is likely to satisfy specialists but will perhaps deter the mythical casual reader. The prose radiates a restless intelligence and wit that cannot always surmount the incredible complexities of contractual arrangements.

And at times, despite abundant detail, it is not always clear what is happening. What exactly did Boris Pregel do to warrant criminal charges being laid against him? The terminal date of 1960 may well be the end of two cycles in the company's history and an appropriate date at which to halt an already fat book, but it is nonetheless stopping in the middle rather than the end. The full transition to a civilian market is left hanging.

H. V. NELLES
York University

LATIN AMERICA

JAMES LOCKHART and STUART B. SCHWARTZ. *Early Latin America: A History of Colonial Spanish America and Brazil.* (Cambridge Latin American Studies, number 46.) New York: Cambridge University Press. 1983. Pp. x, 480. Cloth $29.95, paper $14.95.

The intent of James Lockhart and Stuart B. Schwartz is to write a long-overdue synthesis of colonial Latin American history that incorporates the most recent research not only in history but also in the social sciences. In doing so, they propose to provide a unified view of the Spanish and Portuguese parts of America and to concentrate on internal developments in the Iberian New World with relatively little attention to external influences.

The periodization of the book is fairly traditional: the Iberian and Indian background, the conquest era, the emergence of "mature" colonial societies, and late eighteenth-century changes, plus an epilogue, "The Coming of Independence." The subjects treated in the several parts are also familiar. The authors depart from convention, however, in the way in which they incorporate commonly known as well as newly uncovered information into their own schema of structures and processes. First, Lockhart and Schwartz describe the essential compatability of the basic features of preconquest Iberian and Amerindian societies, which greatly facilitated the establishment of Spanish and Portuguese dominion, and the way in which amalgamation established the basic patterns of colonial societies. Next, the authors compare the differences in the rate and degree of amalgamation, which were determined by the different levels of indigenous culture and kinds of exploitable wealth encountered by the Europeans in the several parts of the New World and which created three main types of cultural and economic areas—the central, the transitional, and the fringes. Then, the authors demonstrate the emergence of two cultural worlds in Spanish America—the Iberian, which was urban centered, and the Indian, which was largely rural—and show the centrality of sugar and African slavery in forming Brazilian society. Finally, the authors consider internal forces rather than external pressures to be primary in shaping late eighteenth-century reorientations in the Ibero-American colonial world.

In the main, Lockhart and Schwartz have accomplished what they purposed. Their book is indeed a long-overdue synthesis of colonial Latin American history, incorporating the most recent research. The way in which its central concepts are developed and interrelated provides an easily comprehensible and plausible view of the Iberian colonial world. As might be expected of two mature and respected scholars, the intellectual level of the book should suit the requirements of persons seriously interested in Latin American history. It falls somewhat short, however, of unifying Brazilian and Spanish Ameri-

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can colonial history. What unification is achieved is through scattered comparisons. But, in fairness, I do not think full unification could have been accomplished except by artificial contrivance.

I have a few quibbles with the authors over matters of fact and interpretation, but my main criticism of their book is that their concern with impersonal structures and processes tends to sanitize their history. From reading it, I got no impression of what colonial Latin America was really like. This study does not tell us, for example, that the conquest was a black and bloody business or that Spanish American Indians, although they survived and maintained their identity, lived a pretty miserable existence.

LYLE N. MCALISTER
University of Florida

KEITH A. DAVIES. *Landowners in Colonial Peru.* (Latin American Monographs, number 61.) Austin: University of Texas Press. 1984. Pp. x, 237. $22.50.

The steady flow of monographs on Spanish colonial landholdings began almost three decades ago and refuses to be halted. And well it should not! François Chevalier's work on the great estate of Mexico (1952) focused on the key element that has continued to influence Latin American social history. Keith A. Davies continues the tradition by examining the coastal region around Arequipa, Peru, occupied early by the Spanish and developed mainly as a grape-growing region. It is clear that land was the axis around which colonial economic and social life turned.

But Davies's focus is slightly different. Whereas land studies usually concentrate on the regional effects on indigenous peoples, Davies's primary interest is how these patterns affected non-Indians. And the pattern was not one of large estates versus Indian smallholders but one of medium-sized holdings dispersed equitably among non-Indians. Land patterns in the valley around Arequipa stood in sharp contrast to the central valley of Mexico and even the central coast of Peru. The great estate was not the major economic unit in this portion of the rural world. In fact, I strongly suspect that further research will uncover evidence that the large landed estate was the exception and not the rule in the colonial Latin American countryside. In any case, Davies studies the nature of landholding in the Arequipa region (about three hundred miles south of Lima) and examines the effects of these holdings on Spanish society by showing how economic gain was intertwined with social ambition.

The string of valleys in the Arequipa region had little arable land even in pre-Spanish times, and the Spanish occupied the area in the 1540s mainly as a back door to Cuzco. The economy was based on tribute collection, but the decline of the Indian population opened more land to settlers, and black slaves were purchased to supply a steady labor force. Despite the destructive earthquake of 1604, grape growing and wine making dominated the rural economy throughout the seventeenth century, for markets existed and the land was well suited to viticulture. Income from the wine industry enabled settlers to send their sons to Spain, build appropriate rural homes, and provide dowries for marriage. Landholdings were consolidated and rural property provided the means to marry well.

This is a major study that further illumines the rural world of colonial Latin America. Davies uses his sources imaginatively and well, painting a rich picture of rural life on the fringes of the Spanish colonial empire. Comparisons with Mexico and the rest of Peru enable one to draw a broader picture and to attempt further generalizations.

NICHOLAS P. CUSHNER
Empire State College
State University of New York

CHRISTOPHER ABEL and COLIN M. LEWIS, editors. *Latin America, Economic Imperialism, and the State: The Political Economy of the External Connection from Independence to the Present.* (Institute of Latin American Studies Monographs, number 13.) Dover, N.H.: Athlone, for the Institute. 1985. Pp. xiv, 540. $52.00.

This book, originating in a seminar convened in London, critically examines dependency and underdevelopment interpretations of Latin American political economy since independence. A section called "The Debate" presents contrasting perspectives. D. C. M. Platt condemns dependency theory as "unsupported by fact" (p. 38). Philip J. O'Brien defends "creative insights" (p. 65) of the "classic" *Dependency and Development in Latin America* (1979) by F. H. Cardoso and E. Faletto. In an overly difficult outline, Ernesto Laclau opposes "class reductionism" and promotes "concepts of democratic and popular 'positionality'" in analysis of "hegemonic struggle" (pp. 70, 89). The volume offers eighteen well-annotated examples of recent, mostly English, scholarship; they recommend revision of overly simple explanatory models. These eighteen essays are organized chronologically for the periods of early statehood, "classical imperialism," the interwar years, and the post-1945 "new order."

A general introduction and summary prefaces, written by Christopher Abel, Colin M. Lewis, and Rosemary Thorp, to each unit are more than adequate to orient and guide alert readers familiar with the contours of the subject matter and emerging

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debates. Coverage is consistent with the editor's list of themes: "the structure and complexity of external connections; the importance of the state and political economy; [and] the emergence of organized labour and the associated processes of social differentiation" (p. 20). Authors feature Argentina, Brazil, Chile, and Mexico in ten essays. Four of the six on the post–World War II era treat large topics: the international division of labor, finance and capital accumulation, primary exportation, and relationships between transnational corporations and national agricultural sufficiency.

It emerges from this provocative collaborative work that complexity, rather than sharply dichotomous theory, provides a sound basis for understanding political and economic change in Latin American countries; it yields more moderate and diverse generalizations than authors like Raúl Prebisch and André Gunder Frank proffered two and three decades ago. The external connection, writer after writer argues, did not necessarily produce unallayed subservience to outside dominance; often it also yielded some benefits in investment, modernization, and growth of progressive social forces and ideas. Thus, strong implications for social action and public policy are not readily apparent.

Good implications for further primary research and theorizing abound. High standards are set by many of the essays. For example, Anthony McFarlane discusses benefits and significant costs of Colombia's early national integration into the international economy, from 1819 to 1875. Charles A. Jones interprets impacts of British investment in Argentina from 1862 to 1914. Lewis urges correction of the view that railways in Argentina and Brazil in the period 1870–1929 impeded industrialization. Ian Roxborough challenges Guillermo O'Donnell's "bureaucratic-authoritarian" model in an examination of multinational firms, the state, and labor in Brazil and Mexico since 1945. E. V. K. FitzGerald analyzes contemporary external finance, changes in domestic investment, and state policy in six major states.

The authors value national sovereignty as the norm. Yet the book's title is misleading, for it suggests a positivistic approach uncharacteristic of the articles. There is a good array of statistical tables and maps, a bibliography of related scholarly work, and a useful index. Rigorous proofreading would have eliminated annoying errors in the text.

ROBERT N. SEIDEL
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MANUEL MORENO FRAGINALS *et al.*, editors. *Between Slavery and Free Labor: The Spanish-Speaking Caribbean in the Nineteenth Century.* (Proceedings of the Conference on Problems of Transition from Slavery to Free Labor in the Caribbean, 1981; Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1985. Pp. xvii, 294. $30.00.

This book consists of papers, most of which were presented at a June 1981 conference held in Santo Domingo. In them, a group of Cuban, Dominican, Puerto Rican, and North American scholars examine the transition from slavery to free labor as it transpired in the Spanish Caribbean during the late nineteenth and early twentieth centuries (roughly 1870–1920). The papers also deal in varying degrees with the changes in the production of sugar and with the entry of foreign capital, which influenced, and was influenced by, the new labor system and the technological advances in the production of cane sugar. Underlying this entire exercise is the notion that the Spanish Caribbean both differed from the islands colonized by the other European powers and possessed characteristics that were eminently comparable (p. xiii).

That the organizers and sponsors of the conference were able to bring together Cuban and United States scholars of varying ideological persuasions and have them present papers on "neutral ground," in Santo Domingo, was in itself an achievement. Yet the editors of this volume decided not to publish the commentaries made on the papers, an unfortunate decision given that some of the conflicting conclusions must have led to informative exchanges of opinion.

Rebecca Scott's study of Cuban abolition and Patrick Bryan's work on labor and sugar production in the Dominican Republic are outstanding. Scott mounts a powerful challenge to Manuel Moreno Fraginal's thesis, to wit, that Cuban slavery collapsed principally as a result of its internal economic contradictions. Whether one agrees with it or not, Scott's well-written presentation is too formidable to be ignored by future students of the question, no matter what their ideological persuasion. The uniqueness of Bryan's study lies in its brilliant insight into the issue of race and color prejudice as it manifested itself during the chronological period that the book encompasses. To be sure, most of the authors mention issues of race and color in their essays, but they tend to pass over them with undue haste or imply that such problems were rooted in the economic class structure. What Bryan convincingly demonstrates is that in the Dominican Republic, at least, economic arguments can not serve as all-encompassing explanations for race and color conflict. Furthermore, his study suggests that the authors dealing with Cuba and Puerto Rico should have paid more attention to these problems.

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Noteworthy also are the contributions of Frank Moya Pons, Franklin Knight, and especially Sidney Mintz. In writing one of the best summaries for a book of essays this reviewer has seen in years, Mintz does not shrink from answering the tough questions. Did Cuba, Puerto Rico, and the Dominican Republic possess common characteristics that made them eminently different from the Caribbean islands colonized by the other European powers? Mintz would appear to agree with this premise. But he also concludes that in the matters in question—the transition from slavery to freedom, the technological advances in sugar production, and the gradual emergence of new labor organizations—the differences between the Spanish Caribbean islands were as great as any of the similarities (p. 271). Obviously, Mintz believes the conference presentations represent a significant contribution to the field of Caribbean studies. I concur with such a conclusion but with this observation: future conferences of this kind will yield little of value, unless studies are presented documenting the processes of change in the French, Dutch, and English Caribbean as well.

LESLIE B. ROUT
Michigan State University

RODNEY D. ANDERSON. *Guadalajara a la consumación de la Independencia: Estudio de su población según los padrones de 1821–1822*. Translated by MARLO ANTONIO SILVAO. (Temática Jalisciense, number 3.) Guadalajara: Unidad. 1983. Pp. 176.

This slim volume contains a wealth of information about a major Mexican provincial city at the time of independence. Using careful sampling and a computer analysis of the manuscript census of 1821–22, Rodney D. Anderson reconstructs the city's neighborhoods and the lives of its inhabitants, their migrations, family and household structures, occupational patterns, and social stratification. Although the text generally sticks close to the data, Anderson also uses previous censuses and various kinds of city records to show how Guadalajara changed during the late colonial period, and he places his findings in the context of the growing literature on contemporary cities in Latin America, Europe, and the United States.

In most respects, Guadalajara fits the patterns that are emerging from studies of other Latin American cities. Like these, it grew dramatically during the late colonial period, its geographically mobile population lived in diverse household arrangements, artisanal production was declining, and women were prominent in family and economic life. The population pyramids, numerous charts, tables, and maps provide a useful basis for exploring comparisons in greater depth.

The book is occasionally marred by a confusing presentation of the data and by an overly literal translation from the English original. Some of Anderson's conclusions (for instance, that Peter Laslett's view of the dominance of nuclear families in early modern Europe does not apply to Guadalajara) stretch the evidence and are only partially convincing. In a few places he contradicts himself, as on the issue of whether intermarriage between ethnic groups was common (p. 157) or rare (p. 145). And the omission from his tables of unknowns, which were extensive in neighborhoods where the census takers failed to provide complete information for everyone, makes it difficult to weigh the significance of some of his findings.

Still, Anderson's book is a valuable contribution to both the colonial and modern history of Latin America. Although the general reader may find it hard to follow the statistical data, the specialist in Latin American demography, urbanization, and social and economic history will welcome this solid study.

SILVIA M. ARROM
Indiana University

CRAIG L. DOZIER. *Nicaragua's Mosquito Shore: The Years of British and American Presence*. University: University of Alabama Press. 1985. Pp. x, 269. $32.75.

Geographer Craig L. Dozier bases his study of Nicaragua's Caribbean shore on first-hand accounts and research in British and U.S. archives. His superb narrative covers the years from 1502 to the present and would read like a novel if not for his constant use of the passive voice. He details the connections between regional, national, and international politics, examines the implantation of Anglo-American culture in the region, beautifully describes the coast's physical characteristics, and explains the difficulties of establishing civilization in an area that, he says, has a past but not much of a present.

Dozier documents the British and U.S. quests for profit along the Mosquito coast. He traces the fluctuations in the fortunes of two commercial centers: Greytown, at the entrance to the San Juan River–Lake Nicaragua route across Central America, and Bluefields, the gateway to the Escondido River banana lands. Over half of the book treats the nineteenth century, when Central America struggled for national identity and confederation, the British established a protectorate (1843) over the Mosquito shore to control the transisthmian waterway, and U.S. interests sought a Nicaraguan passageway to California's gold mines. The author investigates the role of U.S. investors in the region and William Walker's mercenary activities there and analyzes

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why Britain relinquished its protectorate in 1860 and created an autonomous Mosquito Indian Reserve under Nicaraguan sovereignty.

During the 1890s, Yankee investments in the Bluefields area rose significantly as the U.S. Congress debated the merits of canal routes across Panama and Nicaragua. By the turn of the century, U.S. firms controlled Bluefields and the plantations on the Escondido. Major U.S. banking and military interventions in Nicaragua began between 1909 and 1912, and by the 1920s the Standard Fruit Company had deprived Mosquito Indians of their lands. Dozier characterizes the Mosquito coast of the 1920s as a backwater composed of moribund cities and of villages engaged in subsistence agriculture where Moravian Protestant Indians spoke English, ignored the Spanish culture, and repeatedly threatened to secede from Nicaragua.

When the Somoza dynasty came to power in the 1930s, it regarded the Mosquitos as alien. The Mosquitos welcomed the Somozas' salutary neglect, shared their anticommunism, gave little support to the guerrillas who ousted the dictatorship in 1979, and rebelled when the Sandinistas decided to develop the eastern coast and improve living standards there. The Sandinistas naively expected the Mosquitos, as part of the oppressed class, to covet social change. Instead, the CIA induced some of the Indians to join the counterrevolution.

Despite Sandinista efforts to convert Bluefields into a deep-water harbor and commercial center that would benefit the entire coast, Dozier holds little hope for reconciliation between the Mosquitos and the Sandinistas. Historically, cultural and ideological barriers and mutual intransigence have impeded cooperation. Currently, the major obstacle to overcome is not governmental repression, as the author implies, but the wedge driven between them by a United States bent on destroying the Nicaraguan revolution.

This timely volume provides essential background reading for those of all ideological persuasions who seek a more thorough comprehension of the history of the relationships between Nicaragua's Hispanic and Mosquito societies.

SHELDON B. LISS
University of Akron

LINDA ALEXANDER RODRÍGUEZ. *The Search for Public Policy: Regional Politics and Government Finances in Ecuador, 1830–1940*. Berkeley and Los Angeles: University of California Press. 1985. Pp. xv, 281. $32.50.

The nineteenth century witnessed a period of substantial state building in Ecuador, a phenomenon evident in expanded national budgets and increased state-provided goods and services. Thus, the coup of 1925, long considered by scholars to have ushered in a major reorientation in state-society relations, should be understood as an event with evolutionary roots in the nineteenth and early twentieth centuries. This is the central thesis of *The Search for Public Policy*, a book that presents significant events in Ecuador's development in this century against the backdrop of the previous one.

Linda Alexander Rodríguez has written about an important topic in an understudied country. Using data on national and regional revenues, exports, budgets, and finances, she indicates the extent to which Ecuadorian governments of the nineteenth century grappled with the problems of regionalism and the lack of political consensus resulting from geographic fragmentation and a poor natural resource base. These problems meant that governments were constantly threatened by conflict over control of the state apparatus and its patronage opportunities, were chronically short of money, and were increasingly expected to provide goods and services to elite and middle-class sectors of the population. The projects that could be provided by an activist state became an attractive spoil to regionalist interests; the state that emerged in this context was activist but weak, losing to the regions its capacity to direct national development. The close relationship between the government and local banks, especially those of Guayaquil, resulted from the fiscal weakness of a state that was ineffective in generating revenue through taxation and a country that was unattractive to foreign investors. Instability characterized the period, but Rodríguez demonstrates that this did not mean lack of policy or consistency in fiscal practice, only significant limitations on the government's ability to carry them out effectively. Coastal elites, particularly banking interests, were the beneficiaries of this situation.

The coup of 1925, which initiated a period of highland hegemony, centralizing development policy, and greater coherence and effectiveness in government action, was a more successful attempt to achieve what earlier governments had desired. The policy and institutional reforms introduced after 1925 as a result of the Kemmerer mission, however, did not survive the depression, and traditional methods of government financing were returned to in the 1930s. Ultimately, the constraints of geographic fragmentation and lack of natural resources, in addition to the impact of the depression, also limited the accomplishments of this era.

This book is a welcome addition to the literature on Ecuador, a country that has recently emerged as an important focus of study among social scientists. In fact, Rodríguez could have expanded her narrative usefully. She might have told us more about how the Ecuadorian experience fits into a compar-

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ative Latin American experience of the same period, for example. What can be learned from a study of Ecuador about nineteenth- and twentieth-century state building, regionalism, and the military's role in Latin America generally? Similarly, the author might have provided her readers with more detail about who, where, what, when, why, and how. In effect, she tends to oversynthesize her material, leaving out interesting details from debates, addresses, journals, newspaper accounts, and related sources. Moreover, although Rodríguez uses quantitative data cautiously, recognizing the difficulties of drawing substantive conclusions from dispersed, incomplete, and inaccurate records, she needs to indicate how inflation, devaluation, and other factors influence the longitudinal data she presents. Despite these criticisms, it is a measure of the important themes addressed in the book that one wants to know more, not less, about the historical development of public policy in Ecuador.

MERILEE S. GRINDLE
Harvard Institute for International Development

BÉATRICE ZIEGLER. *Schweizer statt Sklaven: Schweizerische Auswanderer in den Kaffee-Plantagen von São Paulo, 1852–1866.* (Beiträge zur Kolonial- und Überseegeschichte, number 29.) Stuttgart: Franz Steiner. 1985. Pp. 466. DM 68.

Students of nineteenth-century Brazil have long been interested in the efforts to import European migrants to replace slaves on the coffee plantations of São Paulo. Might such workers have led eventually to a self-sufficient rural population, a yeomanry, instead of the illiterate and relatively helpless rural population left by slavery? About mid-century, both the imperial and the São Paulo governments offered to subsidize the immigration of European rural workers. The German states and Switzerland proved a good source of immigrants because of the economic and political upheavals there. By 1856, two thousand Swiss and two thousand Germans had come to Brazil under the official colonization schemes. Béatrice Ziegler's monograph is a definitive study of the Swiss role in this saga. The author has consulted all the relevant archival sources, both in Switzerland and Brazil. Her sure-handed analysis merits translation into Portuguese.

Ziegler gives a valuable survey of the mental, economic, and social world of the Swiss who left for Brazil. A key incentive was the poor economic prospects in Switzerland, but no less important were the positive images of Brazil and the pro-emigration attitudes of Swiss industrialists, who feared an overabundance of the poor in Switzerland. Once in Brazil, the Swiss struggled to maintain their sense of community in the face of a country far different from what they had expected. Ziegler examined records for all the areas where the Swiss were located. She finds that the immigrants were everywhere locked in battle over how much they owed their plantation owner (and sponsor) for transportation, supplies, and so on and over how fast they were paying what they owed. The Swiss were supposed to be sharecroppers (*parceiros*) until they repaid their debt; then, most hoped to become independent farmers. But this was hardly in the interest of the planters, who, since the abolition of the slave trade in 1850, wanted a permanent new labor source.

The planters' determination to squeeze out the last cent drove the Swiss immigrants to flight, alcoholism, and collective resistance. The most famous confrontation was on the Ibicaba plantation of Senator Nicolau Vergueiro, a wealthy São Paulo planter and immigration enthusiast. Ibicaba seemed a model case of successful immigrant adaptation to all but the Swiss. By 1856 they had grown so distrustful of Vergueiro's accounts that they protested to the Swiss consul and to every authority they could reach in Switzerland. They set off a wave of bad publicity for Brazil and its means of recruiting European immigrants. As a direct result, Prussia prohibited all emigration to São Paulo in 1859.

Why did the colonization schemes fail? In *Rio Claro* (1976) Warren Dean argues that the planter mentality could not abide the thought (or the reality) of having to negotiate with free men; thus, the planters reverted to slaves, who presented no political or psychological challenge. Ziegler suggests a different answer. First, there was a straightforward clash of interests: the Swiss wanted economic independence, and the planters wanted obedient workers. In the end, bad publicity and governmental prohibitions dried up the European source of immigrants. The planters then turned to the interprovincial slave trade for labor. It was not the planters' mentality but their pragmatism that made them give up on the subsidized immigration of Europeans.

THOMAS E. SKIDMORE
University of Wisconsin

JOSÉ PASTORE. *Inequality and Social Mobility in Brazil.* Translated by ROBERT M. OXLEY. Madison: University of Wisconsin Press. 1982. Pp. xviii, 194. $32.00.

Brazil has traditionally had one of the most unequal distributions of income in the world. In the decade 1960–70, which included the "miracle" years of rapid economic growth, the distribution of income became even less equal. Little evidence indicates that incomes have become more equal during the 1970s.

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These characteristics of Brazilian society have bolstered its image of having a rigid social structure.

In a landmark study of a developing country, José Pastore shows that increasing income inequality cannot be equated to lack of opportunity for social mobility. Using a national sample taken in 1973, he obtains data on the occupational history, education, and migration experience of about fifty thousand male heads of household and the occupational status of their fathers. Although the author acknowledges sources of sampling bias, his data are by far the best available for Brazil and compare favorably with those used in mobility studies of advanced countries. With this database, Pastore is able to track trends in intra- and intergenerational mobility in Brazil for much of the twentieth century.

Following the standard methodology (see Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* [1967]), Pastore groups 259 occupations into six major classes. Most of his analysis focuses on intra- and intergenerational flows among these six classes. In addition, occupations are scored on a continuous index, which reflects income and education.

In chapters 2 and 3, Pastore disguishes between two modes of mobility. Circulation mobility refers to inter- or intragenerational movement across classes given a fixed occupational structure, as in a game of musical chairs. Structural mobility refers to movement into new occupational niches created by structural change. Although he does not attempt a detailed explanation of the emerging occupational distribution in Brazil, Pastore reviews the latest studies in chapter 4. Clearly, jobs have grown more rapidly in services requiring high skill than in agriculture. The transformation of Brazil's occupational structure, away from agricultural toward industrial-service employment is a common aspect of development. Hence, economic development creates considerable opportunities for structural mobility.

In continental-sized Brazil, opportunities have grown more rapidly in industrial São Paulo and in the frontier states rich in resources than in the agricultural regions of the northeast. Both structural and circulation mobility, then, are associated with a high degree of geographic mobility.

From this conceptual and methodological starting point, Pastore elicits several extremely interesting findings. First, the rate of social mobility in Brazil is comparable to that in most advanced democracies. Second, about 80 percent of the mobility in Brazil can be accounted for by structural change, which is a higher percent than in more stable countries. Third, there is considerable intergenerational mobility into and out of the uppermost class. Fourth, the main mechanism of intergenerational status transmission is through access to education.

Although Pastore does not draw many explicit generalizations and policy inferences, his findings suggest a new interpretation of the increasing inequalities in income. Opportunities have increased differentially faster in occupations requiring higher skills. One route to equalization, then, is the upgrading of the educational opportunities available to the lower classes.

It is a pleasure to read a work on Latin American sociology that is neither overly theoretical nor quantitative for its own sake. Pastore deftly mines his data to address an answerable research question cogently, without swamping the reader in numerology. A well-translated edition, the book's only weakness is a few typographical errors in the formulas and an inconsistency in the definition of "total mobility" on pages 31 and 32. These peccadilloes aside, Pastore's monograph is essential for scholars interested in economic development and social stratification.

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CHE GUEVARA. *Guerrilla Warfare*. Foreword and Case Studies by BRIAN LOVEMAN and THOMAS M. DAVIES, JR. Lincoln: University of Nebraska Press. 1985. Pp. xiv, 440. Cloth $22.95, paper $10.95.

Among Cuban revolutionary leaders, Ernesto "Che" Guevara was unique in discerning that the Cuban Revolution could be a powerful influence in promoting insurgency elsewhere in Latin America. The romantic Guevara, whose fiery spirit wedded Don Quixote's impulsive faith in the power of individual will to Karl Marx's strict postulates for the development of a revolutionary situation, proclaimed that revolutionaries themselves could create the revolution. Yet Guevara's 1960 manual, *Guerrilla Warfare*, hardly matched "the staccato singing of the machine-guns" that his followers heard in Guevara's powerful rhetoric. Broadly derivative from other revolutionary theorists and closely focused on tactics, *Guerrilla Warfare* was further limited in its application with Che's death in Bolivia at the hands of United States–trained Rangers. Indeed, U.S. emphases on internal security and counterinsurgency effectively seemed to limit the range of Che's stirring call to arms until the dramatic Sandinista triumph in Nicaragua in 1979.

Editors Thomas M. Davies, Jr., and Brian Loveman go well beyond a reissue of Guevara's tactical manual. In a comprehensive introductory essay they place Guevara's ideas in historical perspective, concluding that his ability to develop a Marxist praxis within a Spanish American political context and his "inspiration and faith in ultimate

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victory," rather than his contributions to revolutionary theory, constitute Guevara's major legacy to Latin American revolutionaries. Because the book stresses guerrillas rather than Guevarism in general, the editors spend less time developing Guevara's ideas, which derive from the tradition of José Enrique Rodó and José Martí, than they do in documenting the swift responses that the U.S. made in combating the guerrilla through the promotion of "civic action" and other forms of counterinsurgency designed to rob these movements of peasant support. Still, the authors note that Guevara's stirring anti-imperialist vision ultimately came to be accepted by non-Marxists as well. The failure of both Guevara and the U.S. to accept nonaligned revolutionary movements, the editors contend, has produced a legacy of terrorism and military repression throughout Latin America in the wake of Guevara's death. The fact that even today's debt crisis is viewed as an East-West, rather than an American, issue attests to the durability of Guevara's legacy among U.S. policy makers.

Guerrilla Warfare's tactical focus was directed against "Caribbean-style dictatorships" but was subsequently broadened to include formal democracies, which Guevara contended masked the domination of the capitalist class. The editors describe the savage theoretical debate that broke out among Latin American Marxists after 1967 and reduced the *foco*, or guerrilla band, to a method of struggle rather than a political apparatus as Guevara had envisioned. In a series of seven comparative essays the editors demonstrate how the major rural-based Latin American guerrilla movements conformed to Guevarist ideas and methods. A helpful political chronology and extensive endnotes are useful for the general reader.

The reissue of *Guerrilla Warfare* and Loveman's and Davies's essays on the development of guerrilla warfare throughout Latin America are welcome additions to the scholarly literature on Guevarism and seem to support the idea that Guevara erred in attempting to extend his movement outside of targets such as those offered by dictatorships exemplified in Cuba, Nicaragua, and the Dominican Republic to include governments in Peru, Colombia, Bolivia, and Venezuela, where later efforts failed to oust democratic administrations. Even in Nicaragua, which the editors include as a postscript, the question of Guevarist influence is confused by the towering presence of Augusto Sandino, who in many ways vies with Guevara as the progenitor in Latin America of this type of warfare. Those interested in the Latin American guerrilla experience will find this well-conceived and well-organized book an indispensible addition to the literature on a time period and a form of armed struggle that was considered a liberating and purifying human experience.

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JAMES D. HENDERSON. *When Colombia Bled: A History of the* Violencia *in Tolima*. University: University of Alabama Press. 1985. Pp. 352. $30.00.

La violencia is the term Colombians use for the violence that persisted throughout much of their country between the late 1940s and the mid-1960s. In form, it was a virtual civil war among partisans of the country's two historic parties, the Conservatives and the Liberals, although no real armies took the field and no explicit, centralized leadership existed on either side. Its participants, as well as its victims (some two hundred thousand in two decades), were mainly peasants.

Sociologists and political scientists have written most of the previously published work on *la violencia*. They have sought to explain the movement's causes, nature, and impact at the national level, effectively treating it as a single phenomenon even while acknowledging that it took on distinct forms and intensities at different times and in different parts of the country. Historian James D. Henderson instead chooses an approach that differs from most others in two critical respects: first, he centers his attention on one department (Tolima) and one community within that department without, however, neglecting the national context; second, he largely eschews the social, economic, and psychological dimensions of *la violencia* in giving us straight narrative history and emphasizing almost exclusively its political origins and development.

The research is prodigious, with sources including government documents (regional and national), interviews, a variety of newspapers, and family archives. The writing style is lively, as are the colorful minibiographies of local elites and guerrilla leaders, in all helping to convey a real sense of *la violencia*, its setting, and its historical antecedents. An array of appendixes, maps, and photographs measurably enhances the value of the book. Not least, Henderson treats Liberals and Conservatives even-handedly; in fact, he gives more of a hearing to the Conservative side than do most accounts.

Henderson's study has two principal limitations. One is his failure to deal with those causes of violence that go beyond traditional Liberal-Conservative hatreds and such derivatives of the violence itself as revenge. Indeed, he aims a number of gratuitous barbs at social scientists for their attempts to address such questions as the relevance of class, education, and involvement in the market economy

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to an explanation of the violence. He briefly attends to such matters using data from the municipality of Líbano (pp. 224–29), but the definitive study of *la violencia*, when it is written, will have to confront such issues more fully.

Second, and relatedly, we are told too little about why *la violencia* was particularly intense in Tolima, and in certain parts of Tolima, and why it was virtually nonexistent in areas such as the region of the Atlantic coast. Again, he makes some brief comparisons (see, for example, appendix B), but a more extended treatment would have enhanced the significance of the author's portrayal.

Still, the reader concludes *When Colombia Bled* with a deeper sense of what *la violencia* meant, and how it developed, in one region and one community. This, after all, was the author's primary objective, and his book accomplishes that aim better than any other previous study.

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MICHAEL FLEET. *The Rise and Fall of Chilean Christian Democracy*. Princeton: Princeton University Press. 1985. Pp. xv, 274.

CARMELO FURCI. *The Chilean Communist Party and the Road to Socialism*. London: Zed; distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. xiv, 204. $26.25.

Chilean politics have been the focus of a number of scholarly debates that go beyond the concern of regional specialists. Unlike most Latin American or Third World nations, Chile (prior to the present military regime) was a longstanding parliamentary democracy, its parties aligned along class lines and the ideological divisions among socialists, Christian Democrats, and communists paralleling similar patterns in Western Europe. The overthrow of the democratically elected socialist government by the military, with the support of the U.S. government and civilian Christian Democratic and other conservative groups, raised fundamental issues concerning the compatibility of radical structural changes and a parliamentary system. These two studies reflect attempts to come to grips with two of the major contending civilian forces, the Christian Democratic and Communist parties, and their efforts to reorganize Chilean social and political institutions to serve their respective social bases and ideological preferences.

Michael Fleet's work is oriented toward demonstrating that Chilean Christian democracy was a multiclass party, essentially directed by and based on the petite bourgeoisie yet containing a sector of the working class supportive of the leadership's policies of "developmentalism," anti-Marxism, and bread-and-butter unionism. Operating from a "modified class analysis" approach—modified by the operation of political and cultural factors—the study draws largely on secondary works and secondary analyses of electoral polls to delineate the "rise and fall" of the Christian Democrats. The study demonstrates that the incapacity of the Christian Democrats to join forces with the Left was an important factor in undermining their reform efforts in the 1960s; their alliance with the Right was instrumental in destabilizing the Allende government in the early 1970s. The increasing polarization of politics under the military regime is perceived as furthering internal divisions among the Christian Democrats and preventing them from forming a coherent regime if and when a transition to democracy occurs. The strength of this study is found largely in its descriptive accounts of the shifting policies and internal divisions of the Christian Democrats. At the theoretical and analytical level there are numerous problems and debatable assertions and assumptions.

First, the author characterizes the Christian Democratic party as petit bourgeois largely through the simplistic notion that the class position of the majority of a party leadership defines the classes that it represents. This crude instrumentalist conception of the state fails to examine adequately the larger structural and historical issues relevant to characterizing a party. The Christian Democrats' historical origins stem from the Conservative party and are rooted in a strong defense of large-scale capitalist property. From the 1930s to the present the Christian Democrats have supported capitalist modernization, not a return to small-scale individual ownership. During their term of office they defended and promoted foreign investment in manufacturing, and later they violently opposed Allende's attempts to replace capitalist ownership with worker self-management. All political leadership groups in Chile are predominantly petit bourgeois (except the Communists), some representing labor, others capital—a point Fleet fails to examine.

The second weakness is the study's failure to examine the structural ties and linkages between the Christian Democrats and the U.S. government between 1964 and 1973. The links between the CIA and the Agency for International Development as well as numerous other agencies were extensive and played a substantial role in Chilean politics, not only in funneling funds and providing organizational support to the Christian Democrats but also in reinforcing anticommunist attitudes among the rank and file. The material available on this linkage between external state agencies and internal social and political forces is extensive and central to any serious analysis—but absent from Fleet's discussion.

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Third, Fleet's discussion of the Christian Democratic party's support of the military's violent overthrow of Allende relies mainly on immediate political and psychological factors—partisanship and political antagonism—setting aside the long-term structural antagonism rooted in the system of property-ownership and control. Christian Democratic support for a counterrevolution that destroyed all of Chile's democratic institutions and resulted in the dissolution of thousands of trade unions is discussed in the most cursory and superficial manner, as are the substantial reforms enacted by the Allende government.

In contrast to Fleet, Carmelo Furci argues that the overthrow of Allende's democratic government was a product of irreconcilable class conflict and the incapacity of the Communist party to change its commitment to the peaceful road to socialism. Furci's book is essentially a description of the evolution of Communist party policy, focusing particularly on the years after 1950. His thesis is that the Communist policy of broad alliances and electoral strategies was successful in securing the presidency in Chile but was incapable of sustaining it in a period of growing social polarization.

The major weakness of Furci's study is the absence of any detailed account of the level of labor-capital conflict, which would provide the necessary context for evaluating the tactics and strategies of various party leaderships. The critical evaluations seem to be based ex post facto: the wisdom of hindsight replaces systematic analysis of historical conjunctures and specific configurations of political forces. Chilean labor combined support for "reformist" demands and parties proposing structural changes. Whereas Fleet emphasizes the former (and thus exaggerates the degree of moderation of the Chilean electorate), Furci emphasizes the ideological commitments of the labor electorate, ignoring the importance of the evolving social struggles in shaping the revolutionary process. Both studies, however, agree on the increasing radicalization and polarization under the Pinochet dictatorship—factors that weaken the hegemonic position of "moderate leaders" in both parties.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ERIC FRY, editor. *Rebels and Radicals*. Boston: George Allen and Unwin. 1983. Pp. xv, 216. Cloth $22.50, paper $12.50.

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REVIEWS OF BOOKS

TO THE EDITOR:

In his dismissive review of Will Morrisey's *Reflections on de Gaulle: Political Founding in Modernity* (*AHR*, 89 [1984]: 1340–41), Professor John F. Sweets does a serious injustice to a substantial work of scholarship. In a word, he reviews a work of political philosophy as if its author intended to write a history.

Sweets begins by miscounting the book's parts; there are six, not five, a number Sweets got from glancing at the obviously misprinted table of contents. Small wonder that Sweets finds it "difficult to discern an interpretation [of de Gaulle's thought] except for the obvious conclusion that there were elements of modernity and tradition in de Gaulle's ideas." It is in the Sixth part that Morrisey gives his concluding thoughts on de Gaulle's founding.

"De Gaulle's founding": I could not find those words in Sweets' review. Nowhere does he show the slightest recognition that this is what the book is about. Its subtitle, which should give even the most casual browser a solid clue, is lost on him.

In his preface, Morrisey states clearly that he will examine the Gaullist founding "from the perspectives of textual interpretation and political thought." He never says a word about historical research and never claims to be a historian. Yet Sweets unwarrantedly assumes that Morrisey is failing to do something Morrisey never set out to do. So, for example, Sweets objects to the chapters in which Morrisey presents the arguments of various political thinkers who write on "themes intimated by de Gaulle," themes related to the problem of political founding in modernity. "On many occasions the reader is not informed whether de Gaulle actually read or referred directly to the thinkers presented for reflection." This would be a great fault in a historian. But Morrisey is not tracing historical *influences*. He is providing a *theoretical* frame—albeit one solidly grounded in the thought of past philosophers—for de Gaulle's ideas. To deride this as "a game of charades" and "analogy as a substitute for analysis" is as embarrassing a piece of supercilious pedantry as I ever expect to see in a respected scholarly journal.

Two-thirds of the way into his review, Sweets finally discerns a few interpretations, or rather alleged misinterpretations, by Morrisey. The problem, we then learn, is not that Morrisey makes no interpretations, but that he does not make a "critical interpretation." Once again, Sweets ignores the Sixth part, where Morrisey judges the Gaullist founding a failure, which sounds rather critical to me. Specifically, Sweets claims that Morrisey says "de Gaulle's plans and ambitions for France were fixed from the mid-thirties," thus "reject[ing] the interpretation of Stanley Hoffmann . . . who describes a gradual broadening over time of the Gaullist vision." What Morrisey actually says may be seen in a footnote on page 113: "Professor Hoffmann tends to see de Gaulle's plans for France as having broadened gradually as the war (World War II) continued, whereas I think it's clear from *Vers l'armée de métier* that de Gaulle even then required nothing less than a re-founding of France." This refers to Morrisey's earlier, detailed textual interpretation of *Vers l'armée de métier*, a book published in 1934. Morrisey *quotes* the call for a "national refounding" to be "commenced by the army," a commencement not to be effected by a *coup d'état* but by a series of army reforms designed to enable the army to serve as "resource and leaven" to "rejuvenate France." Hoffmann does not consider this passage in his study, *Decline or Renewal? France Since the 1930s*. The passage does not prove that *no* broadening of de Gaulle's "plans and ambitions" occurred during the

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war, but it does show that de Gaulle's *fundamental* intention was indeed "fixed" much earlier. There can be no question of retrospective dating here. Here and elsewhere, Sweets confuses basic Gaullist *principles*, established early and permanently, with Gaullist *strategy*, which changed according to circumstances. Morrisey does not make this mistake.

Sweets predicts that *Reflections on de Gaulle* "will be disappointing to historians." As a student and teacher of history, I was not disappointed, primarily because I did not presume that I was reading history. There is no reason historians cannot learn from other disciplines, but first we have to understand their own preoccupations and methods. The failure to do so is *unhistorical*.

DAN NIMROD
Quebec, Canada

PROFESSOR SWEETS REPLIES:

It is my practice in reviewing books to begin with the author's statement of purpose in order to determine how well he has accomplished his objectives. In the preface (p. ix) to his book, Will Morrisey writes: "Each of the book's *five* parts begins. . . ." He then outlines a First, Second, Third, Fourth, and Fifth part, noting that the Fourth part discusses political founding. It is perhaps pedantry to note that the *author* of the book shared my error and that of the table of contents. Confusion and error are seldom far apart. I am not sure whether Mr. Nimrod has clarified or confused this matter *for both author and reviewer* by pointing out that a Sixth part is indeed indicated on page 133. Let Mr. Nimrod be assured, my evaluation was based on a careful reading of the *entire* book. All scholars who write books, be they political philosophers, sociologists, historians, or from any other discipline, will be judged by how convincing their arguments are to their readers. I take the responsibility of reviewing books very seriously, and I would never consciously "deride" anyone's work. I stand by my assessment of the book in question.

JOHN F. SWEETS
University of Kansas

TO THE EDITOR:

Richard Blanke's review of the book I co-authored with Titus Komarnicki, *From Versailles to Locarno: Keys to Polish Foreign Policy, 1919–1925* (*AHR*, 90 [1985]: 976–77) compels me to make the following observations:

1. We did not try to "explain away" the German majority vote in the Upper Silesian plebiscite of 1921 by references to German terror and British economic greed, although the first is well documented (the second was not a factor in our opinion). Our view—and we are not alone in this—is that the German majority was due to German "outvoters," that is, those born but not resident in Upper Silesia. Polish insurrections which, as we point out, were supported on the whole by the French, helped document the presence of a resident Polish-speaking majority but did not decide the issue. The final settlement, which was so bitterly resented by Germany, was worked out, as we show, by the League of Nations Council on the basis of both ethnic and economic considerations.

2. No reputable historian could possibly suggest, as I am charged with doing, that Danzig only became German after 1871. Unfortunately, much of the historical background to this, as to other chapters, had to be cut to the minimum, but I still cannot see any attempt to suggest that Danzig was Polish (pp. 91–92). Thus, the native Danzig population that was outnumbered by an influx of German settlers after 1871 refers to the native German-speaking Danzigers. This later led to differences of opinion between local merchants and the bureaucrats who came from Germany on the policy to follow in trade and general economic relations with Poland.

3. Professor Blanke charges the authors with embracing "the untested assumption that any attempt by Poland to coexist with her two powerful neighbors was senseless and could only be achieved at the price of her independence." What we point out is that this contemporary Polish perception was solidly based on recent bitter experience with both German and Russian domination, and that the Poles of that time suspected that any concessions to either neighbor would inevitably lead to more demands and then to the end of independence. With respect to what Professor Blanke calls "reasonable proposals," for example, autonomy for East Galicia, we do not turn to public opinion "as a court of last resort" —nor, indeed in the case of the Polish stand on frontiers. What we do say is that public opinion, that is, public perception, is a key factor in understanding the policies of democratic countries, and Poland was a democracy at that time. From this point of view, it is quite unrealistic to fault the Polish statesmen of the time for not "testing" the possibility of buying better relations with Germany or Russia by making territorial concessions or of trying to work out a real system of autonomy for East Galicia.

4. Professor Blanke's statement that the Poles have been paying "a terrible price" since 1939 for their policy failures up to that time is based on two implied assumptions: a) that it can be proved conclusively that other policies would have averted disaster, and b) that the "sins" of the fathers are justly visited upon their children. The first is clearly

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an assumption that cannot be proved; moreover, it ignores what we already know of the aims of Hitler and Stalin. The second is less a historical than a moral judgment based on preconceived beliefs. It smacks more of Calvin than of Ranke. (One may well wonder whether the other East European peoples, which suffered in the war and now groan under the Soviet yoke, are also paying for the "sins" of their fathers, at least as far as Professor Blanke is concerned.)

The aim of our book was to encourage Western historians to rethink the standard negative evaluations of Polish prewar policy by showing Polish perceptions of key problems, British and French perceptions of Poland, and the interaction between Polish, British, and French policy. From this point of view, I take some comfort from Professor Blanke's admission that the book may prompt some adjustments in the prevailing views of prewar Poland.

ANNA M. CIENCIALA
University of Kansas

PROFESSOR BLANKE REPLIES:

In brief response to the points raised by Professor Cienciala:

1. The German majority in the Upper Silesian plebiscite was not due to the participation of "outvoters" (a Polish demand), which merely increased that majority from 53 percent to 60 percent. Decisive was the fact that one third of the Polish-speaking voters opted for Germany, that is, they spoke Polish but "felt" German. I think we should respect their choice and accept it as part of the historical record and not continue to seek grounds for denying its validity (which is what I meant by "explain away").

2. There is no mention in my review of Danzig "only becoming German after 1871"; what I did write is, I believe, a fair reflection of pages 91–92.

3. That interwar Polish leaders and public opinion alike were quite nationalistic and saw little possibility of getting along with their principal neighbors is not at issue. But does this require today's historian to share their perspective in the face of all the questions which hindsight permits (requires) us to raise? Is it necessary (or possible) to "prove conclusively" that different policies would have worked better when one can scarcely imagine a less favorable outcome than 1939? Of course Poland has been dealt a difficult hand, but, as Bismarck observed, a politician must play the hand he is dealt; neither he nor posterity can blame fate or circumstances for all that goes wrong.

4. The reference to "sins" and Calvin suggests a complete misunderstanding of my remarks; sins of fathers are *not* justly visited upon their children, which is why historians should call the fathers to account more rigorously than do the authors of this work.

Again, I would not want disagreement on individual points to obscure the opinion which I hope I made clear in my review: that this is overall a work of impressive scholarship which makes its case clearly, tenaciously, and (at times) persuasively; it was rewarding to review and deserves a wide readership. But where the authors deal with such controversial issues and make so little secret of their national-political sympathies, it should come as no surprise to them that many readers who do not share their perspective are likely to react much as this reviewer did.

RICHARD BLANKE
University of Maine

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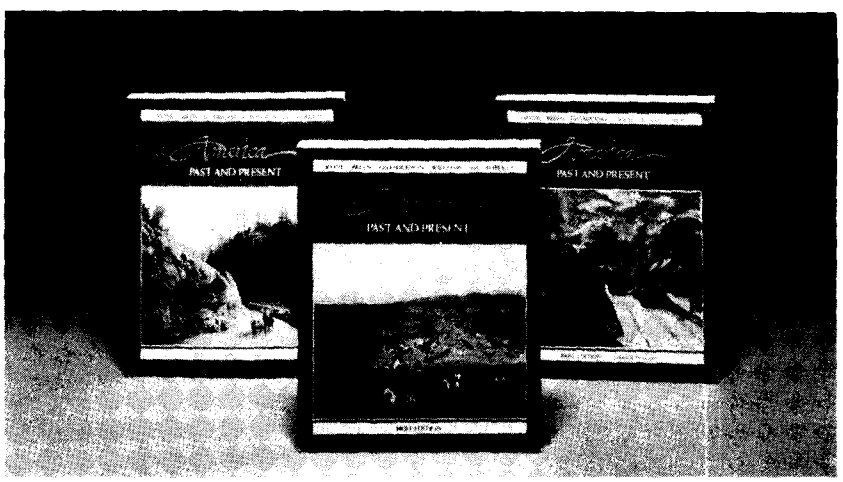
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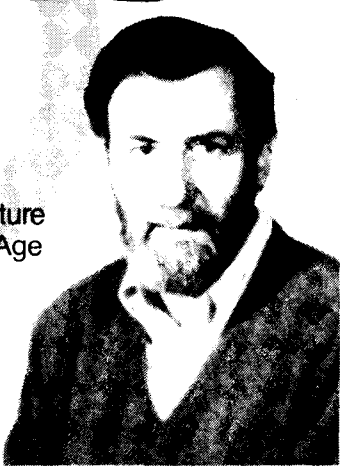


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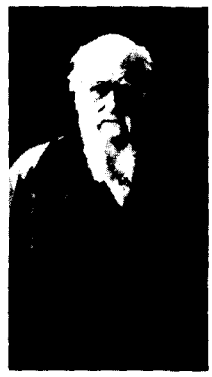


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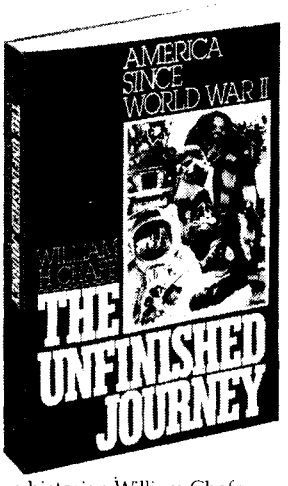
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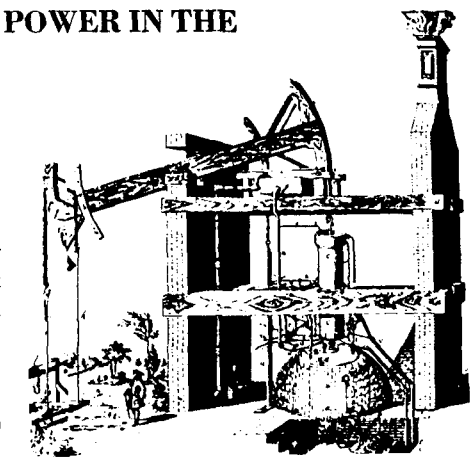
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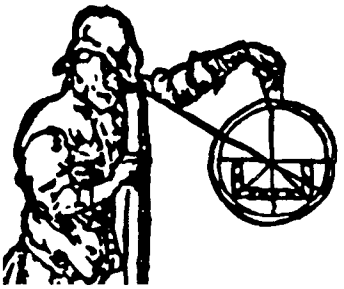
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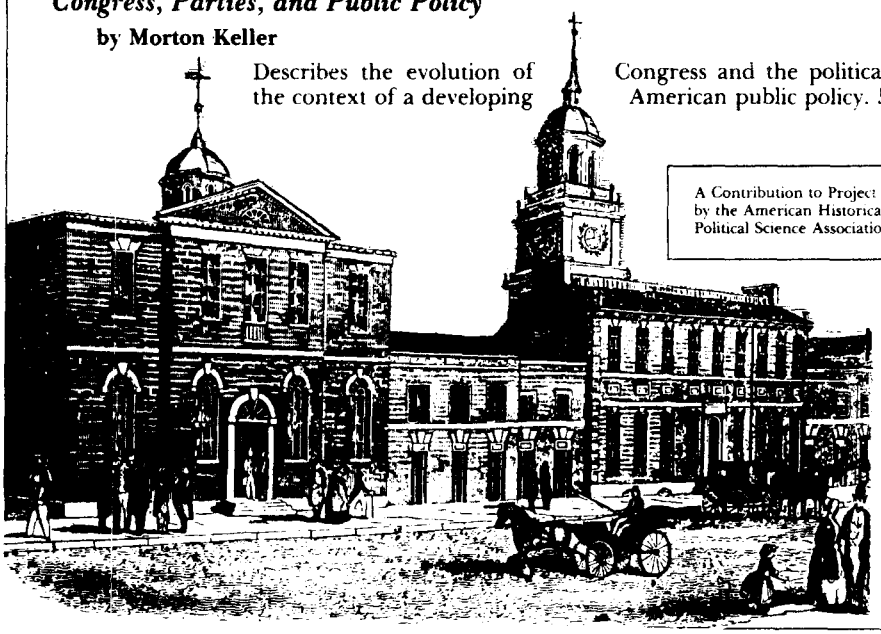
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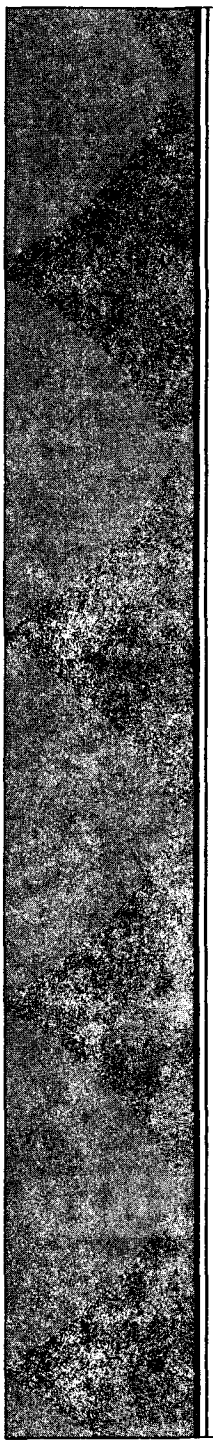
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